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(An. M. Magill, R. M. Patterson)
From
THREE LECTURES

ON THE

ORIGIN AND PROPERTIES OF

MALARIA OR MARSH MIASMA,

**WITH THE BEST MEANS OF PREVENTING ITS FORMATION AND OF
OBVIATING ITS EFFECTS ON THE HUMAN CONSTITUTION,
WHEN THIS CANNOT BE DONE.**

DELIVERED TO THE MEDICAL CLASS

OF THE

UNIVERSITY OF VIRGINIA.



BY

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PRUDENCE, IN THAT INSTITUTION.**

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ON MALARIA.

Causa latet, vis est notissima.

THE subject of Malaria, or marsh effluvium, has for a long time attracted much of the attention of the Medical Profession, and interested to a high degree the feelings of society at large. Physicians of all countries have tortured their brains, and exhausted all the known resources of the Chemical art, in attempting to discover the properties of this invisible agent of disease and death, which prostrating in its silent march whole cities and communities, bids defiance to all the measures hitherto suggested to stay its destructive progress. Unquestionably it is one of the most important subjects which can be presented to the contemplation of man; to detect the causes which originate; to ascertain and define the laws which regulate, and to discover means by which the human family may be shielded from this "pestilence which walketh in darkness," would form a suitable and noble employment for the Philosopher and Philanthropist, as well as the Physician.

None of all the many "ills which flesh is heir to," has probably been so prolific of destruction and misery to the race of mankind, as miasmatic exhalation.* In all quarters of the world except the frigid and ice-bound north, where perpetual winter "rules the year," this scourge of humanity, has been occasionally found, breathing disease and death upon all within the range of its morbid influence. In the east and in the west, its withering and desolating effects have been felt in all their terrific severity.† Along the classic shores of the Me-

*It is computed that upwards of one half of the human race perish by fever in one or other of its forms.) Miasma is the principal if not the only cause of febrile disease.

†According to the report of the committee, consisting of Dr. W. Ainslie, President, and Mr. A. Smith, and Dr. M. Christie, the fever which prevailed in the provinces of Coimbatore, Madura, Dindigul and Tinnivelly, in the years 1809, 1810 and 1811, consigned to the grave 106,789 persons, to which may be added the ruin of the constitutions of many thousands.

diterranean it has poured with a liberal hand the "vials of its wrath."* The shores of Africa, that region of mental darkness, though harmless to the natives, are fraught with the seeds of disease to the European and American constitution: Witness the lives that have been and still are annually sacrificed upon the altar of Colonization. The extensive marshes of the Netherlands have often proved the sources of severe and wide-spread disease.† Our own highly favored land almost throughout its entire extent, has frequently been visited by extensive and malignant epidemics, increasing in frequency and severity as we proceed southward. The bold and majestic rivers which flow through our country, and beautify its scenery, often prove the source of destructive epidemics. The "king of rivers," the grand and magnificent Mississippi, like the Nile of Egypt, and the Ganges of the East, while it fertilizes the soil by its extensive annual inundations, spreads at the same time far and wide the seeds of disease.

Miasmatic exhalation as a cause of disease seems to have entirely escaped the notice of the Ancient Physicians. The keen and scrutinizing mind of Hippocrates, confessedly the first among the ancients for talent and observation; and in more modern times, the sagacious intellect and unequalled observation of Sydenham, (both of whom paid especial attention to the causation of fever) failed to detect this prolific agent of disease. The merit of its discovery was reserved for Lancisi, and Italian physician, who, about the beginning of the last

*Of all the countries lying along the Mediterranean, Italy has probably suffered most from Malaria. Rome once the proud "mistress of the world," in her career to greatness and splendour, had often well nigh been annihilated by the blighting hand of pestilence. Livy mentions the occurrence of fifteen plagues before the year Ab. Urbe Condita 59, and no one now doubts that they were produced by the pestiferous exhalations from the neighbouring marshes. One would have supposed that these severe visitations, before she had acquired the bone and muscle of maturity would have given a permanent check to her growth, but the cupidity and enterprise of man can triumph over apparently insuperable difficulties. All that portion of Italy too, which is bounded on the one side by the Mediterranean, and by the Appennines on the other, and extending from Pisa on the north to Terracina on the south, has been rendered a complete desert by the Malaria in which it abounds. This portion of the country (says Griffith) is almost uninhabited, "and the appearance of the few wretched beings whom necessity detains there is frightful in the extreme, exhibiting the effects of the noxious exhalations in every form of bilious disorder." Other countries along the sea coast, have also occasionally suffered from this scourge most severely. In Gibraltar, in 1804, out of a population of about 10,000, there perished from the 1st of September to the 31st of December inclusive, 5946 individuals, being more than one half of the inhabitants. In 1810 another destructive pestilence ravaged the same place which is clearly proved by Dr. Burnett to have arisen from marsh exhalation. In Cadiz in 1800, out of a population of about 60,000, the number of the affected from the beginning of August to the first week in November, amounted to 48,520.

†The famous Walcheren expedition of the British in the year 1809 is still fresh in the recollection of many. Out of sixteen thousand troops, constituting the whole British force, there was on the 23rd of September 9,800 prostrate with the fever.

century published a work on the subject, entitled, *De noxiis paludum, Effluviis, eorumque remediis*, in which its origin and characteristics are investigated with considerable minuteness.

Malaria has a two-fold operation upon the human constitution.—The first consists in the production of active disease, or fever strictly so called; the second relates to that gradual deterioration and undermining of the powers of the system, which is the result of an habitual exposure to an atmosphere polluted by this deleterious principle, by which premature old age, with its usual attendants, imbecility of mind and decrepitude of body, is brought on. This latter has been denominated by M. Monfaleon, the Physiological effect of Malaria. “Man (observes he) in marshy countries, is of small stature, and often deformed from infancy either in the trunk or members, with a remarkable disproportion in the splanchnic cavities. His skin is pallid, and often covered with spots of an earthy appearance. His muscles are flabby, his fat of a watery nature—his hair and beard fair—his complexion pale and sickly looking—his physiognomy exhibiting even in youth, indications of age, as well as of melancholy, apathy and hebetude. At twenty-five the work of decay commences, and the great majority die between the age of 35 and 50 years.”*

“Travellers who have visited and observed the inhabitants of the Pontine marshes in Italy, have drawn a most melancholy picture of these wretched people. They have compared them to spectres, and could not find words to convey the impressions which their appearance made on the minds of strangers. M. Pioney assures us, (continues M. Monfaleon) that in the year 1777, when he visited the centre of the Pontine fens, the greater part of the inhabitants presented such an œdematous state of body, that the impression of the fingers would remain on any part of the body for hours together. Many peasants were seen on the road, apparently asleep. They had as it were ceased to exist. He asked one of the unfortunate sojourners in the Campagna di Roma how he could live in such an unhealthy place?—“We do not *live*,” he replied, “we *die* here.”† Such is the dark picture drawn by those who have witnessed the ravages of Malaria upon the constitution of the unhappy wretches, who from necessity are habitually subjected to its influence. Other writers, mention this slowly pernicious, though ultimately fatal operation of Malaria. Johnson in his invaluable work on tropical climates, remarks—“I have myself, in rambling through the villages of Beveland and Walcheren, been struck with the conspicuous marks of premature old age, which all beyond maturity exhibited, particularly among the peasantry. On inquiring the ages of decrepid wretches, withered, sallow, and appar-

*Med. Chi. Review, October, 1825, p. 321.

†Ibid p. 322.

rently on the borders of four score, I was surprised to find that fifty-five or sixty years were all they had numbered in these noxious fens." Dr. Robert Jackson, physician to the British army during the revolutionary war, says, "we plainly perceive that an habitual exposure to it, is peculiarly *unfriendly* to the principle of life, and in a very remarkable degree shortens the period of existence. * * * * * There is not on record, I am credibly informed, an instance of a person born at Petersburg, Virginia, and constantly residing at the same place, who has lived to the age of twenty-one. When the British army marched through this province, in the year 1781, I had the opportunity of seeing a native of this town, who was in his twentieth year, but he was said to be the first who had ever attained so advanced an age. He was decrepid as if from the effects of time, and it did not appear that he could survive many months. Yet it is not a little curious, that this man had never been much confined with sickness. The residing constantly in the same pernicious air, seemed alone to have been sufficient so remarkably to accelerate decrepitude."*

In the further consideration of this subject, for the sake of convenience, we will treat of it under the following heads:—1st. What are the characteristics of Malaria? 2d. What are the causes which produce it? 3rd. What are the best means to prevent its formation? 4th. When formed, what are the measures necessary to remove or render inert the sources of it? 5th. What are the best means of obviating its effects upon the human system, when the cause cannot be removed.

1st. What are the characteristics of Malaria? Though we are as yet totally ignorant of the precise or intimate nature of Malaria, we are fortunately well acquainted with many of its physical qualities. I say fortunately, because by this knowledge, we are enabled very often to avoid the attacks of this unseen and silent enemy to our health; whose step is as noiseless as the grave, and whose destroying presence is known only by its effects.

The first characteristic of Malaria which strikes us, is its superior density, when contrasted with atmospheric air. If not exposed to the action of a powerful sun, or heat from some other source, it rises but a very short distance from the earth's surface. It has been ob-

*Petersburg in this state, the town alluded to by Dr. Jackson, is situated on the right bank of the Appomatox, which partially overflows every year. There are also some marshes contiguous to the town. This place suffers even now almost every year, more or less from miasmatic diseases. Malaria, however must have abounded much more at the time Dr. Jackson speaks of (53 years ago) than at present, for Petersburg is now quite a large and flourishing city, which could not well be the case, if this cause of disease existed either in the same quantity or intensity now, that it did at that time. We suspect the account given to Dr. Jackson was a very exaggerated one.

served by most writers upon the subject, that domestic animals are the first to suffer from the presence of Malaria, and the fact of the greater exemption from disease, of those who live in the upper stories of houses, where Miasmata abound is well known to those who have paid any attention to the subject. They are not only less liable to disease, but when attacked have it in a milder shape. In proof of this, many facts may be cited. Dr. Ferguson states, that "according to the official returns during the last sickly season at Barbadoes, the proportion of those taken ill with fever in the lower apartments of the barracks, exceeded that of the upper by one third, throughout the whole course of the epidemic."* Dr. Hunter testifies to the same fact in his work on diseases of the army in Jamaica. "The barracks at Spanish-Town," says he, "consist of two floors, the first upon the ground, the second on the first. The difference in the health of the men on the two floors was so striking as to engage the attention of the assembly of the Island, (of Jamaica) and, upon investigation, it appeared that three were taken ill on the ground floor, for one on the other. The ground floor was not, therefore, used as a barracks afterwards.† Bancroft, Pringle and others mention similar facts. It is also capable of being driven by the wind, elevated situations afford on security against the approach of Miasma. Where the ascent is easy and not abrupt or precipitous, it may and has been propelled by the wind to the tops of high mountains. It may be carried, horizontally by the same agent a very considerable distance, amounting, in the opinion of some to several miles. There is, however, in relation to this much difference of opinion. Bancroft limits its sphere of action to "a quarter and perhaps half a mile." We cannot but think however, that he is wrong; he appears to have formed his opinion from the distance at which Malaria ceases to produce disease when driven over a body of water. There can, however, be no doubt, from the multitude of facts on the subject, some of which we will presently mention, that it soon loses its deleterious qualities by contact with water, and therefore, that the distance at which its effects may be experienced under such circumstances, cannot apply to its operation under other circumstances. The Pontine marshes are several miles from Rome, and the fevers which have for ages infested that city and its vicinity, have been universally ascribed to the poisonous exhalations from the above mentioned marshes. "It would be a matter of utility (says Dr. Jackson,) could we determine with any certainty to what distance from its source, the noxious Effluvia extend; but this is a question which we cannot hope to ascertain exactly. It is not uniformly the same in all situations, depending upon the concentrated

*Phil. Journal, No. 13, p. 18.

†p. 306, as quoted by Bancroft.

state of the exhalation at its source, the obstacles it meets with in its progress, and the nature of the ground over which it passes or is directed. I have known its influence very remarkable at the distance of a mile and a half, on the top of a hill of very considerable elevation." *But though it is sometimes thus extended in its operation, it is also occasionally most remarkably and unaccountably circumscribed in its influence. Bancroft states a curious fact of this sort, "being at the Horse Guards on the 19th November, 1810, I saw there Captain McKoy, of the 21st Regiment of foot, who had then just arrived with despatches from Sicily, and was informed by him that, in July and August, 1800, while his own and another company were quartered at the post of Venetico, in a barracks of nearly 100 feet in length, which consisted of one (ground) story, *forty* men of the other company, occupying one half of the barrack, were attacked by a violent Malaria fever, which proved fatal to eleven of them, but did not reach a single man of his own company, occupying the other half of the barrack, though there was no division between the parts or halves occupied by the two companies, nor any perceptible difference in the soil on which the different parts or ends of the barrack stood. Each had, indeed, its own door to pass into and out of the barrack, but both doors opened on the same side;—nor was there any difference in the discipline, diet, or management of the men of the two companies."†

Malaria is deprived of its febrific qualities by passing over even a small surface of water. In what manner this gas is thus robbed of its pernicious properties it is impossible to say, it is most probably absorbed by the water, as we know it has a great affinity for moisture, being most frequently found entangled in fogs and dews. There can however be no doubt of the fact. Bancroft details many striking instances of the immunity from disease enjoyed by those whose duties kept them off the shore, the entire crews of vessels, stationed within less than a quarter of a mile of the shore, where disease was prevailing to a most alarming extent, escaping without the slightest indisposition. Dr. Rush says "the Yellow fever has never been known to pass from Philadelphia to the Jersey shore, and the miasmata generated on the east side of the Schuylkill rarely infest the inhabitants of the opposite side of the river. Many persons found safety from the plague of London, in 1665, by flying to ships which lay in the middle of the Thames, and, it is well known no instance of Yellow fever occurred in those Philadelphia families that confined themselves to ships in the middle of the Delaware, in the year 1793. But three or four of four hundred men, on board of a ship of war called the Jason, commanded by Capt. Coteneuil, perished with an epidemic Yellow fever, in the

*On Fever, p. 263.

†On Fever, p. 167.

year 1746, at St. Domingo, in consequence, Dr. Desportes says, of her hold being constantly half filled with water.”*

Another peculiarity of Miasmatic exhalation, is the acquisition of augmented virulence and activity during the night. Dispersed by the rays of a powerful sun, it remains in the higher regions of the atmosphere during the day, but when he withdraws his vivifying beams, and the heated air begins to cool, no longer sustained by this rarifying principle, in obedience to the laws of gravitation it begins to descend, and finally settles upon the earth in its greatest degree of concentration. Its effects under such circumstances are often appalling. Dr. Ferguson, speaking of the swamps at English Harbor, in the island of Antigua, remarks, that, “so pestiferous was their atmosphere, that it often occurred to a well seasoned soldier, mounting the night guard in perfect health, to be seized with furious delirium, while standing sentry, and when carried back to his barracks, on Monk Hill, to expire in all the horrors of the black vomit, within less than 30 hours from the first attack.” Lancisi the discoverer of this morbid principle, was well aware of the aggravated virulence of this gas, during the night, and thought that a state of somnolency rendered the individuals exposed to it much more susceptible of its bad effects, and in consequence of this belief, earnestly warns travellers against sleeping or even journeying during the night through the Pontine marshes. “*Neque verò solum dormientibus noxius est per noctem palustris aer; sed etiam iis qui vigilantes per cœnosa loca interfaciunt.*” Quæ de re monitos vellem, quotquot vel Neapoli Romam, vel Roma Neapolim contendunt, ut diurnos potius cœtus subeant, quam nocturni frigoris voluptati decepti contemneratam ambientis aeris vim excipiant.”† “Similar admonitions (says Bancroft) are still given at Rome to all strangers, and they are founded on the uniform experience of all ages, which has afforded numerous instances of travellers, who in consequence of their passing these fens during the night, (though the passage requires but six or eight hours) have been attacked with violent and mortal fevers.”§ Johnson in his work on Tropical Climates details some melancholy instances of the dire effects of Malaria after night fall. “In the month of November, 1804, two parties of men, belonging to his Majesty’s ship Tremendous, were employed on shore, at the island of Madagascar; one party, during the night, filling water, the other cutting wood during the day. Four of the night party were attacked with the endemic fever of the country, and three of them died. The whole of the day party escaped the fever, though exposed to an intense sun in the laborious occupation of wood cutting.”

* Rush. Inquiries, vol. 2, p. 130.

† Phil. Journal No. 13, p. 12.

‡ De Nox. Palu. Efflu. p. 80; as quoted by Bancroft

§ On Fever p. 167

About two years after this, his Majesty's ship *Sceptre*, in the same place and upon a similar occasion, experienced a still greater disaster among her watering or night party, to whom the mortality was chiefly confined.

Another instance of mortality unprecedented in the annals of medicine occurred among the crew of his Majesty's ship *Braave*, which sailed from Madras in the month of June, 1800, in company with the ships *Centurion*, *Dædalus* and *la Sybille*, on a secret expedition against Batavia. This vessel at first, "lay at anchor under the small island *Onrust*, about three miles from the main land of Java." Some little sickness occurring among the men of the 12th Regiment, stationed on the island, "the commanding officer of the expedition, conceiving that the vicinity of the island to the main land was the cause of the sickness, * * * ordered the sick to be removed on the 28th of September, to the small island of *Edam*, situated nine miles out to sea; a circumstance that he thought must insure its salubrity." "*Edam*" (says Johnson) "is covered with trees, long grass, and jungle, having a part of the island itself in a stagnant and marshy state." * * * Of sixty soldiers (12th Regiment) landed at different times, *in health*, to do duty at *Edam Hospital*, and other buildings on the island, between the 1st of October, and the 12th of November, thirty one died: Of the remaining twenty nine embarked on breaking up the blockade, twenty two died at sea; the other seven were sent to *Malacca Hospital*, where all, or nearly all of them shared the same fate."

All the soldiers getting ill on *Edam*, sixteen marines were landed from the *Centurion*, to do night duty, as they expected an attack from the Dutch Gun Boats. The whole of these men were seized with the fever, and thirteen died." Such is the sad history of the appalling effects of marsh exhalation when in a highly concentrated state. In this instance too, the mortality was confined to those who spent the night on the island. The surgeon who gives the account of the disease and remarks, that "from our first arrival at Batavia, in August, until our return to Malacca, in January following, we only buried one man of fever, who had *not slept on the shore at Edam, Cuypers, or Onrust island, whereas almost every person, who slept even a single night at Edam died*. No ill effects were experienced from going on shore in the day time, or among the sick at the island, I myself regularly visited the hospital at *Edam* every day with perfect impunity, till one night that I staid rather late, attending the unfortunate surgeon of the *Dædalus*; in consequence of which, I was three days afterwards seized with fever, but recovered by mercury carried to ptyalism. I think it highly probable, however, that had I slept on shore, no medicine would have saved my life."†

* On tropical climates, vol 1, p. 215.

†Ibid. p. 222.

In this account we have a striking exemplification of the fact of the entire dispersion of Malaria by the heat of the sun during the day, and of its subsequent descent at the approach of and during the night.

The period of time during which it may remain inactive, after its reception into the system, is a subject of much difference of opinion, among medical men. Johnson says, "the space of time which intervenes between the application of the poison to the system and its ostensible operation in the form of fever, depends on the degree of its concentration, and the predisposition of the patient. It will for instance, be found in some places so powerful, that a man in perfect health by remaining on shore during the night, in marshy situations, and wet or autumnal seasons, shall have the fever violently the next day, and die on the third or fourth. On the other hand it may be applied in so dilute a state, as to require eighteen, twenty, or even thirty days to bring on a fever; and even then, perhaps, only in consequence of some of the numerous predisposing or *auxiliary* causes concurring to enable the *original* to develop itself. If we take the medium of these two extremes, we shall have the ordinary period, viz. twelve or fourteen days, which elapses between the reception of vegeto-animal Miasmata into the body, and their manifestation in the shape of actual disease."* Lind says that the attack in some is protracted to the 10th or 12th day. The time mentioned by these authors may be, and we doubt not, is, the "ordinary period" at which its action is developed. We think however, that they fix much too short a limit to the time during which it may remain inoperative. We have not the shadow of a doubt that it may lurk unperceived and unfelt in the system for a much longer time, and under the influence of strong exciting causes, may seize its unhappy victim months after exposure to it, and at a time perhaps when he is congratulating himself upon his fancied security. It is most frequently brought into action during the winter subsequent to its reception into the system; the intense cold, and the vicissitudes of this season, powerfully concurring with the lurking Malaria to excite disease; hence sickly winters so often (we might say almost invariably) succeed sickly autumns, the prevalent disease exhibiting a mixed character of summer and winter disease, commonly known by the name of bilious pleurisy, in which we have a local inflammation exceedingly dangerous in itself, added to the usual symptoms of miasmatic fevers.

But though the winter after its reception generally brings it into action, we believe that the time of its incubation may be protracted to a much longer period. Dr. Hunter, in his work on the diseases of the army in Jamaica, relates two striking, and to us convincing, examples in proof of this. The West Suffolk regiment of Militia, was called in

*On Tropical climates, vol. 1, p. 116

1793, from their own county, one of the healthiest in England, to Hilsea Barracks, of which the "low, marshy, and unhealthy situation has been fatally known to the army since their first erection." The men were then "all in perfect health," but "became very sickly ; and twenty two died of fevers before they left the barracks," about the end of June following. In July this regiment, with eleven other battalions, was encamped at Waterdown, in the neighbourhood of Tunbridge Wells, and "their sick list soon amounted to 100 out of 500." * * * "The deaths in this regiment exceeded the amount of those in all the eleven other battalions together, on the *same* ground. But the point of most importance is, that some of this regiment were taken ill of fever in the month of *October*, who had never had it before," nearly four months after their removal from "the cause of the disease, at Hilsea barracks."

The next instance related by him is still stronger. "The 18th regiment of foot, after having been stationed at Hilsea barracks from the 22d June, 1783, to the 9th of October following, was then embarked for Gibraltar, where, though the regiment consisted of only 400 men, the number of agues was increased by the beginning of May, to 280, including women and children—of whom a considerable part were then *recently* attacked for the *first time*, and whilst *no agues existed in any other part of the garrison*." On these facts Dr. Hunter remarks, "that the 18th regiment of foot, and the West Suffolk regiment of Militia, after leaving Hilsea barracks, were both in situations where they could not contract fevers, and the regiment encamped with the *latter*, and those in garrison with the *former* had *no fevers*. There cannot therefore be a doubt, that the poison had remained quiescent in their bodies for four, five, and six months."* On these statements we do not deem it necessary to make any comments; even the most prejudiced and incredulous must admit that the evidence afforded by them is of the most convincing and incontrovertible character.

Extraordinary as these facts appeared to be (says Bancroft) when first made known, the fullest confirmation of them has been since produced by the late expedition to Zealand; in which it has been indisputably ascertained, that considerable numbers both of officers and soldiers, who were employed on that service, and who escaped the sickness, whilst at Walcheren, and other parts of Zeland, were attacked by intermittent fevers, and some of them as late as *six, seven, eight* and even *nine months*, after being brought back to this country; though care was taken to place them generally in situations remote from all known sources of miasmata."† Pringle, also, when speaking of the diseases of the campaign of Dutch Brabant, in the years 1746 and 1747,

* Bancroft on fever, p. 174 and 5. † Ibid. p. 176.

mentions "that the detachments in Zealand and Bergen-op-Zoom, suffered extremely from the bad air of the country, and in reference to this he afterwards remarks that the same battalions, in the beginning of the next campaign, were remarkably more sickly than any of the rest."*

Enough has been said, we think, to establish the point that Malaria may remain in a quiescent state, during a period extending from the time of its first reception into the system to nine months, though the time of its development in the shape of disease is usually much shorter. One reason of this diversity in the operation of Malaria, both as to time and degree, may be found in the different degrees of its concentration when applied to the system. It generally possesses most strength immediately at its source, and is there most speedy and terrible in its operation, but the farther it travels from that point the more diluted it becomes, and the more tardy and milder the disease produced by it. Thus in a large city, or a portion of country, whose atmosphere is polluted by miasmatic exhalation, at different distances from its origin we may see all the grades of febrile disease. Immediately at the 'fons et origo mali,' we may perhaps meet with the malignant Yellow fever; at a short remove from it, we may find the same disease much mitigated in its symptoms; still farther it will be found to produce only a mild remittent, until reaching the boundaries of its influence we shall find the mildest form of fever namely intermittent.

Another reason of this diversity in its effects, consists in the state of the system when exposed to its influence. Constitutions broken up by intemperance, or weakened by fatigue, or exposure to a hot sun, or great degrees of cold, suffer soonest and most violently from Malaria. Thus in an army exposed to miasmata we find the common soldiery invaded to a great degree by disease, while the officers, who are well clothed and better protected from the inclemencies of weather, and indeed, are in a great measure exempt from the operation of the debilitating causes to which the common soldier is exposed, comparatively healthy; and in large cities we find more sickness and more mortality among the wretched paupers, than in any other class of society.

The moon, also, appears from the testimony of highly distinguished and veracious physicians, to exercise a marked influence over the human frame, by which it is rendered more susceptible of the operation of miasmata. We have, heretofore, been a total disbeliever in all such influence—looking upon it as the mere dream of visionary theorists, but candour compels the confession, that our incredulity has been somewhat staggered by the strength and respectability of the evidence in its favour advanced by the different authors, who have contended

*Pringle on diseases, p 121.

for it. Dr. Robert Jackson, than whom no man had a sounder judgment, or more discriminating mind, gives the following account of the manner in which he became convinced of the truth of lunar influence over disease. "When I arrived in Jamaica," (says he) "in the year 1774, I had no other knowledge of the influence of the moon in fever, than what I retained from a cursory reading of Dr. Lind's dissertation. I remember however to have mentioned the circumstance to several practitioners, who had lived many years in the island. As I conceived there was a similarity between the climates of Jamaica and Bengal, I thought it not improbable, that some of the practitioners of the country in which I then was, might supply me with satisfactory information on the subject. There were none of them, however, who acknowledged that they had ever observed any connexion between the moon and febrile diseases; neither were there many of them, who seemed disposed to give credit to its existence. Twelve months or more elapsed without my having paid any further regard to the fact, when an accidental relapse of fever, happening near the time of full moon, recalled Dr. Lind's observation to my memory. It likewise brought to my mind a circumstance which till then I had overlooked. I had seen frequently, though without attending to it particularly, that three or four soldiers of a company of the 60th regiment, who were quartered at Savanna la Mar, and of whom I had the care, were attacked with fever on the same day; whilst it seldom happened that any other febrile illness made its appearance in the garrison for the ensuing fortnight. This having been observed oftener than once, at the time the moon was near full, a hint suggested itself, that the cause which was said to influence relapses in India, might have an effect on the original invasion. But in order to ascertain the truth of this conjecture, I provided myself with the almanac of the year 1776, and marked, in the blank leaf of it, the precise date of attack, of all those fevers which came under my care. In looking over those memoranda at the end of the year, I found that I had put down thirty cases of proper remitting fever, the invasion of twenty-eight of which was on one or other of the seven days preceding new or full moon; that is in the second and last quarters." The next year, "of twenty-eight cases which were found in the almanac, twenty-two were in the periods above mentioned." Dr. Jackson, a year or two after this, came to this country as one of the physicians to the British army, and continued here the same train of observation on this subject, which he had begun in the West Indies, and with a striking confirmation of the results there obtained. Of one hundred cases of fever "which were found marked in the almanac, eighty were found to have commenced in the usual period of invasion, that is, in the second or last quarters of the moon." In conclusion he remarks, that "even in times of greatest epidemic sickness, when the connexion was evident

ly weakest, the number of the sick was generally doubled in the periods approaching new or full moon."

These certainly are striking facts, and *facts* upon this subject are what we want, for it is in vain to reason about it. The strongest objection against the belief of any such connexion between febrile diseases and the moon, at certain periods (and to us it is a very strong objection) is that those who contend for lunar influence ~~over disease~~, do not agree as to the precise stage or period at which the moon is supposed to exercise this noxious agency in the production of disease. Drs. Jackson and Balfour being directly at issue in regard to it. "Non nostrum tantas componere lites."

The question may now be asked, what are the avenues through which this noxious principle invades the system? The cuticle, the internal surface of the nose, the tracheal ramifications, and the mucous lining of the stomach, have each been held forth as the exclusive inlets for miasmata. The advocates of the sympathetic theory, and the disciples of Broussais, contend that the stomach is the first point of attack, while a more enlarged and comprehensive view of the subject would suggest, that all the parts of the body which have been mentioned, may afford the avenues through which Malaria gains admittance into the system. If gaseous substances *can** be absorbed by the epidermis, and by the mucous lining of the respiratory apparatus, and the experiment of Bichat, would appear to establish the fact, both reason and common sense would point them out as the chief inlets for miasmata. "A general idea prevails, (says Johnson) that the stomach is the medium through which the matter of contagion acts; and, by analogy, that marsh miasmata takes the same course. But when we consider, that at each inspiration the atmosphere impregnated with this principle is largely applied to the delicate texture of the lungs, it is not difficult to conceive, that it may pass into the blood, (if it is in any case absorbed) as readily as Oxygen. There are, besides, the Schneiderian, and other membranes of the nares and fauces, to which it must have constant access, while there is but one way for it to pass into the stomach, viz. along with the saliva or food. Further, when we see this principle, in a concentrated state, produce fever in a few hours, with high delirium, can we suppose that it enters the system by the circuitous route of the alimentary canal and lacteal? If it be said that it acts through the medium of the nerves of the stomach, why not through that of

*It was ascertained by Bichat, that the fætor from putrid animal matter gained admittance through the skin. Having discovered that the flatus of his bowels acquired the smell of the air of his dissecting rooms, he respired for some hours through a tube which led into the open air, while he remained in a room containing highly putrid air. The same effect was produced on the flatus, as when he respired the air of the room, and the same effect was produced by respiring the air of the room, through a tube while he remained some hours in pure atmosphere." Parsons on Malaria, American Journal, No 13, p 90.

the olfactories, which is a shorter road?"* The security which rubbing the skin with oil,† is said to afford against the plague strengthens the idea that the contagion or the cause of this feil disease, be it what it may, finds a high road into the system through the pores of the cuticle, and if the contagious essence of plague, (which in the opinion of many is neither more nor less than miasmata) can penetrate the system through this medium, why may not miasmata? Dr. Harrison, of Louisville, Kentucky, in an excellent paper on the epidemic bilious fever, published a few years ago in the Philadelphia Journal, advances a singular argument against the supposition of the cuticle being the medium through which Malaria enters the system, "covered, as the true skin is, (says he) with a comparatively dense, insensible cuticle, which even warm fluids, and friction, with unctuous substances hardly renders permeable, how can an exceedingly subtle aeriform substance penetrate it—or how affect its nerves?" To this we would reply, that miasmata can penetrate the pores of the cuticle, *so much the more easily*, because it is "*an exceedingly subtle aeriform substance*;" if the Dr. had have reversed the question there might have been some reason in it, but as it stands it has certainly no force at all.

What is its *modus operandi* in the production of disease, after it gets into the system? The opinions upon this subject have been and still are as variant and discordant as upon any connected with the science of medicine. The advocates of the doctrine of sympathy, (if it has any advocates now, for it appears to have completely succumbed to its more powerful rival of French extraction,) and the disciples of Broussais, contend that its primary operation is on the mucous lining of the stomach, and that it there produces a local inflammation or irrita-

*Tropical climates, vol. 1, p. 113.

†"Mr. Tully has informed me (says Dr. Good) that there was no instance of an attendant on the infected having received the contagion so long as he was regular in thoroughly illining himself with oil, wearing a dress soaked in oil, or a covering of oil skins. And to the same effect is the evidence of Sir Brooke Faulkner, physician to the forces at Malta, in 1813, before the select committee of the House of Commons, June 14th, 1819, who, in answer to the question, "How were the Military attendants preserved?" replied, "with respect to the Pest Hospital in which I attended, they were, in my opinion, preserved by wearing a dress of oiled silk, which prevented the possibility of any contact of infected matter with the skin, and probably, also, by its promoting free and copious perspiration, and in consequence, preventing absorption,"

"To the same effect it has been asserted by Mr. Baldwin, of Cairo, that among upwards of a million of inhabitants carried off by the plague in Upper and Lower Egypt, during the space of four years, not a single dealer in oil, as far as he could learn, had fallen a sacrifice to it." Study of Medicine, vol. 2, p. 440.

Ferguson also in noticing the almost entire exemption of the negro from the fevers of the torrid zone, remarks, "that one of the most obvious peculiarities of the negro, compared with the European, is the texture of his skin, which is thick, oily, and rank to a great degree; and from this circumstance the theorist when he speculates on the mode of the reception of the marsh poison into the system, whether by the lungs, the stomach, or the skin, may draw a plausible conjectural inference in favour of the last."

tion from which all the phenomena of fever result. The stomach according to this creed, is not only the *punctum saliens* of febrific disease, but is the organ chiefly and prominently affected during its whole course, and that to cure it most effectually our remedies must be addressed to this part, *the sole and whole object*, being to extinguish the inflammation which has been set up in the mucous membrane of this organ.

Waiving the many objections to this theory, which present themselves at its first blush, such as, the incompatibility of the existence of an inflammation of the mucous membrane of so important an organ as the stomach, with the incipient symptoms of fever, viz: paleness, weak pulse, chilliness, and the many other signs of diminished action of the heart, we will proceed at once to the main point in the argument, (i. e. the assumed inflammation of the stomach) and deny the fact of its being always, or even generally present. To support us in this denial we must appeal to the symptoms of fever during life, and to the post-mortem examinations of those who died of fever. Wherever there is inflammation there is, and must be, pain and tenderness of the inflamed part; we find this to be universally the case, in whatever part of the body inflammation is seated; at least we know of no exception to this, and can imagine none, unless it can be proved that some parts of the body are destitute of nerves. Apply this general rule to fever, and what a number of cases do we meet with, in which there is not the slightest tenderness on pressure—nay, we may often press the epigastrium, until the back bone can almost be felt, and no complaint of pain or soreness is made by the patient. We will admit that certain parts may be inflamed, and when perfectly quiescent and undisturbed, may not give very great pain; but if the same parts be roughly handled or compressed, the groans and contortions of the patient will tell how much he suffers. Now even granting what the disciples of Broussais claim when they say, *Il a demontre que les intestins possedaient fort peu de sensibilite*, they must indeed be entirely dead to feeling, not to betray some sensibility, when severely compressed. We have made the existence or non-existence of this symptom, a point of special inquiry in the great number of cases of bilious fever, which we treated during the last fall, and do not now recollect a single instance in which it was present, whether early or late in the disease.

Again, how seldom do we see even a loose state (not to say diarrhæa) of the bowels, in febrile affections? On the contrary, have we not much more frequently to contend with a dangerous torpor of the bowels? Should this be the case if the mucous membrane of the stomach and bowels was inflamed? What takes place, when the mucous membrane of the nose, trachea and bladder is inflamed? is there

not an increase in their respective secretions? and in dysentery when this same membrane is known to be inflamed, do we not perceive the same effect? It is incumbent, therefore, upon the friends of the doctrine, to show why this symptom should not be present in all cases, of what they call gastro-enteritis.

We have elsewhere insisted upon the soundness of this argument, and we still think it not only solid, but unanswerable. What are the symptoms which present themselves, when the mucous membrane of the stomach and bowels is in a state of inflammation? Is not a diarrhea one of the most prominent and invariable in its occurrence? Listen to what the ablest author of modern times says upon this subject. I mean Southwood Smith, of London, whose treatise on fever, well deserves the high encomiums which have been lavished upon it. He mentions a *purging of the bowels* as one of the most invariable attendants on, or symptoms of fever, when the mucous membrane of the stomach and bowels is inflamed; and he, let it be recollected was in the constant habit of connecting symptoms during life, with the appearances after death.

If we examine the symptoms of fever as described by Sydenham and his successors down to the promulgation of this doctrine, we shall find *no one* so constantly present and of so predominant a character as to claim special notice from them. If any one symptom was more frequently present than another, it was that of great determination of blood to the brain, as indicated by headache, &c.* Can we therefore do so great injustice to the memory, the talents and sagacity of the many great men, who have shone as bright stars in the medical firmament during the last century and a half, as to suppose that a symptom, which is now pronounced to be so uniformly present as to constitute the *ipse morbus*, (and which if so invariably present now, must have existed then,) should have escaped their observation and vigilant attention?

The practice too, which has been found most successful in combatting fever in all parts of the world, is irreconcilable with the idea of inflammation of the stomach and bowels, as constituting an essential and invariable ingredient in the pathology of fever; it has consisted essentially of active depletion, by venesection and continued purgation, the latter of which is now reprobated by Broussais and his followers, as pernicious in the extreme, and as having a tendency to increase the local inflammation. Sydenham followed this practice and contended with great success, against the fevers of his day by this course of treatment,

*Dr. Crampton, in his medical report containing a brief account of the late epidemic in Dublin, gives the following estimate of the relative proportion in which different parts of the body were affected in fever. Out of 755 cases, 550 complained of the head, 129 of the chest and 76 of the abdomen, and what let me ask, are symptoms, but signs of internal disease?

for he expressly states that “*the method of cure above delivered, which consists in bleeding and purging, is the most effectual one to conquer most kinds of fever;*”* and in later times, Hamilton *was forced* into an acknowledgment of the invaluable efficacy of purgatives even in Typhus fever, then considered a disease of debility. The ferocious diseases of the East and West Indies, can only be met and vanquished, by this practice, pursued with a single eye, and the most unwavering energy. Our own great Rush, (whose memory ought to be dear to every American Physician,) triumphed over the Yellow fever of 1793, and of every subsequent year of its appearance in Philadelphia, by venesection and free purging with the most powerful articles of the *Mat. Medica*. He adopted this mode of treating the disease, after having tried every other then in vogue, and pursued it with the steadfastness and determination of one who possessed the *mens conscia recti*, regardless of the obloquy and slanders which were heaped upon him; and the result was an unparalleled degree of success, and a rich harvest of fame and gratitude from the numbers whose lives were saved by this practice; and if we take the collected experience of the Physicians scattered over the wide expanse of our country, we shall find a vast preponderance in favour of depletion by venesection and free purging in the management of fever.†

The theory of Broussais utterly fails to account for the phenomena of Intermittent fever. Here we are left to the absurd supposition, that there is an intermittent inflammation, which suddenly begins and progresses to a certain point, and then as suddenly subsides, only to be regenerated after the lapse of some definite time, to run again the same course. We would now ask, “what kind of inflammation must that be, which explodes, as it were, the moment the clock strikes a particular hour, and vanishes the moment the clock strikes another hour—and thus for days or weeks together? What kind of inflammation must that be which every second day, terminates in a *profuse perspiration* from head to foot, and yet is renewed, after an interval of 48 hours, with the same symptoms as before, and so on?”

The symptoms of fever, therefore, are not even generally, still less always, such as to warrant the belief that the mucous membrane of the stomach is inflamed; and the recorded post-mortem examinations of those who died of fever, corroborates the deductions drawn from a

*Wallis' Sydenham, vol. 2, p. 344.

†We do not object to the practice of Broussais; so far as it goes, we think it excellent. Spare diet cannot be too much recommended, and leeching, if blood enough is drawn, may do very well, though we certainly should prefer drawing it *pleno rivo* from the arm. But we should as soon think of attempting to bale the water from a leaky and sinking ship, with the hollow of the hand, as to trust *alone* to leeching and *eau sucrée* in the treatment of severe fevers.

view of the symptoms during life. Autopsic examinations of those who die of fever generally, indeed reveal the traces of inflammation, deep and indelible, in some part of the system, and there cannot be a doubt, that this in ninety-nine cases out of a hundred is the cause of death from fever. Sometimes the brain and spinal marrow are found to have suffered from the disorganizing effects of inflammation, in some cases the viscera of the thorax present the marks of fatal inflammation: and in others the bowels; in many cases inflammation is found to have existed in two of these vital portions of the system. The brain and thorax, the thorax and bowels, and the latter and the brain, are often found to have suffered conjunctively, and in a few cases, post-mortem examinations, reveal the fact that the brain, thorax, and bowels have all suffered from inflammation; but we deny, that this inflammation is discovered more frequently or as frequently in the stomach as in the brain. Southward Smith, in his work on fever, gives an account of 110 dissections, 51 of which, he ranks under the head of Cerebral disease, 12 under that of Thoracic, 26 under that of Abdominal, and 21 under that of "mixed disease." Here we have, within one of double as many cerebral cases as abdominal. Some of the cerebral cases, were, it is true, complicated with abdominal, and sometimes thoracic disease, and vice versa, but many of them show an entire exemption from inflammation of the mucous tissue of the stomach and bowels. In these cerebral cases too, which were complicated with inflammation of other parts, the symptoms of brain affection were predominant and first to occur; and yet we are to be told in the face of such facts, that inflammation of the mucous membrane of the stomach exists *in every* case. Such reckless assertions it appears to us, can only proceed from minds irreclaimably biassed and blindly devoted to a particular theory or opinion.

In the December number of the *Medico-Chirurgical Review*, for the year 1823, is a review of M. Tacheron's *Pathological Researches*; five dissections, of what were considered genuine gastro-enteritis, with the symptoms before death, are there brought before the eye of the reader. In all these cases post mortem examination revealed the existence of inflammation in the *brain, stomach, and lungs*. *The symptoms in all*, during life, indicated a high degree of *cerebral excitement*, and the indications of disease in the *stomach* were comparatively slight and unobtrusive. The second case is a very curious one, and as the reviewer remarks, shows in a strong light, "the rage for system which pervades even those who swear they are entirely free from prejudice." A young man aged 17, was seized with clearly marked symptoms of inflammation of the brain, which put an end to his life in eleven days. Dissection—the vessels of the brain greatly gorged with blood—the *right hemisphere disorganized by a sanguineous infiltration*, so that the

natural substance of the brain could not be recognized. Two table spoonsful of effusion at the base of the brain. In the abdomen some small tubercles were observed on the intestines of a reddish colour."

Reviewer's remarks: "Thus, then, a youth of 17 years of age, comes into hospital with unequivocal symptoms of cerebral disease, and nasal hemorrhage. He is treated first with tamarind water, and, as the symptoms increased, with the decoction of cinchona. He dies, and one hemisphere of the brain is found completely disorganized, (which is a mere bagatelle)—but "*des petits tubercules d'une couleur rouge*" are found in the intestines—and lo! the great French "*doctrine pathologique*," is confirmed! Half a brain destroyed, sinks into insignificance, when compared with the tubercles in the intestines."

If the disciples of Broussais exhibit *such cases* as the above, to support the French doctrine, well may he exclaim, "save me from my friends," they certainly would relieve the opposers of the doctrine from the trouble of searching for evidence against it.

But however frequently, and in whatever organs, inflammation may be found after death to have existed, we think that with unprejudiced minds there cannot be a doubt, that it is the effect and not the cause of the exalted arterial action of fever. Neither is it an *invariable effect* of fever, because there are many mild cases of fever which, from their commencement to their termination, present no evidences or symptoms of this state; it is, indeed, a *mere contingency* which may or may not happen during the progress of a fever. If inflammation therefore does not exist in every fever, it follows that it is not *essential* to fever, and therefore cannot be an *invariable cause* of this state. In many cases, too, in which it supervenes during the course of an idiopathic fever, its first symptoms, its gradual and steady progression, increasing with the increase of the arterial action which originates it, and vice versa, evinces it to be a secondary and not a primary affection.

Again, in symptomatic fevers strictly so called, if you remove the *cause*, you at the same time remove its effects, (i. e. increased action the heart,) "*sublata causa, tollitur effectus*." Thus in pleurisy, for example, we find the febrile action to diminish *pari passu* with the symptoms of local inflammation: not so, however, in miasmatic diseases, though every symptom of local inflammation, wherever seated, may have disappeared, still the fever pursues its course, checked, but not subdued, rendered harmless but not entirely vanquished. The primary symptoms of fever, weak pulse, chilliness, shrinking of the features and surface of the body, are incompatible with the idea of a *then existing* inflammation of the stomach or any other part. But when the heart reacts and the blood is propelled with increased velocity through every fibre of the system, we are then likely to see inflammation developed in whatever part is weakest, or most predis-

posed to it. It may be either in the stomach, brain, or thorax, or in any two of them, or perhaps, all three of them may suffer. We have a striking example of this in our winter epidemics or bilious pleurisy, in which the fever, brought into action by the lurking malaria, develops inflammation in the lungs, which are notoriously predisposed to disease at that season of the year.

These are some of our objections to the French doctrine of fever. If it were a mere matter of opinion unconnected with *effects*, we should deem it of no importance, but believing that when carried into practice, it is fraught with evil consequences, we shall consider it a paramount duty, (feeble as is our opposition) to contend against a theory which in our opinion is not only radically wrong, but practically inefficient.

The question of the *modus operandi* of malaria, again presents itself. We are decidedly of opinion, that we must look to the blood for the first operation of malaria. However painful the retrogression may be to some, we shall have to travel back to the old and much scouted humoral pathology for the truth in this matter. Far be it from us to advocate the many ridiculous notions and glaring absurdities, connected with this subject in "olden times," but we do think that our rejection of it has been too indiscriminate, and that amongst the *disjecta membra* of this antique edifice, something valuable might be here and there picked up; in sweeping away the rubbish we have swept away much that was good.

That the blood of those exposed to miasmatic exhalation is vitiated, is obvious from its dark colour, and from the depravation of all the secretions. If the fountain is polluted, the streams which proceed from it *must* be impure. The dark sallowness of the skin, the furred or dark tongue, the unpleasant breath, the black and offensive discharges from the bowels,* the high coloured urine, and foetid sweats, all unequivocally indicate a contaminated state of the vital fluid. The doctrine which attributes fever to a local inflammation of the stomach, and what may be called the sympathetic theory of fever, is directly opposed to the idea of a vitiated or depraved state of the blood; the latter theory, indeed, is based upon the supposition, (erroneous and unfounded,) that no foreign substance ever reaches the circulation with a preservation of its characteristic properties. We presume, however,

* These are common in miasmatic districts, even where the individuals are apparently well, and can almost always be brought away by taking medicine; and an attack of fever is always cured with most certainty and facility, when these discharges come away most copiously and easily. A fever very often will not yield at all, until they are obtained. No matter how copious and frequent the discharges of another sort may be, they are in many instances, attended with no relief, but as soon as dark evacuations are obtained, there is a great and marked amelioration of the symptoms. Rush as well as many others mention this.

that even the most prejudiced must now surrender this point; the great number of experiments recently made, proving beyond a doubt, that medicines and other substances not only may, but actually do enter the blood *unchanged*; and if so “gross and palpable” a material as medicine, why may not gaseous substances or malaria. We know (from the experiment of Bichat already mentioned) that the fœtor of animal substances in a state of putrefaction, reaches the blood, and reasoning from analogy, it is fair to suppose that other aeriform substances do the same. Our opinion, then, of the *modus operandi* of malaria in the production of fever, is succinctly this. When mingled with the blood, it either directly or through the instrumentality of the nervous system, produces a weakened action of the heart, from which results that paleness and shrinking of the surface, which are most generally observed at the onset of fevers, and which is directly the reverse of that flushing of the skin and fulness of the features, which is seen during the exacerbation of fever, when the heart’s action is exalted and vigorous. This diminished action of the heart soon works its own cure, for in consequence of it, a more than usual quantity of blood, must necessarily accumulate about that organ, which being its natural stimulus, soon rouses it from its depressed state into preternatural activity, according to the intensity and duration of which, we have the different forms or types of fever. If the reaction be violent, and any part of the system from previous disease or other cause be weakened, we shall most probably have a local inflammation, superadded to the existing excitement of the heart and arteries; or if it be mild, we may have a fever only of “simple excitement.”

Of the Chemical nature of Malaria. Various and vain have been all the attempts heretofore made to ascertain the constituents of this active poison to the human frame; it remains, as yet, one of nature’s arcana; but may we not indulge the hope that practical chemistry will one day add to its already great and brilliant achievements, that of the discovery of the intimate nature of Malaria.

But though nothing is positively known of the chemical nature of Malaria, many conjectures have been hazarded in relation to it, none of which in our opinion can be sustained. Dr. Cooke, Professor of the Theory and Practice of Medicine, in the Transylvania University, suggests that Malaria is nothing more nor less than carbonic acid, and supports the idea by a very close and ingenious analogy between the known effects and qualities of fixed air and those of Malaria. But there are many and insurmountable objections to their identity. 1st. If carbonic acid gas be the cause of fevers, they ought to prevail in all cities and towns *every winter*, as it “is produced in immense quantity by the combustion of wood.” 2d. The numerous distilleries in our country, where an immense quantity of this gas is extricated, should

prove so many foci of disease ; but we know that not only those who live near them, but those who live in them, are not peculiarly subject to fever. 3d. It is often taken internally, in considerable quantity in the shape of soda water, effervescent draught, &c., without any febrile effects. 4th. There is not, (as far as we can discover) on record a single case of fever following the resuscitation of the many who have suffered asphyxia from imprudently going into an atmosphere of fixed air.

Dr. Faust, in a late essay on Malaria,* labours to prove that carburetted hydrogen in combination with vapour, is the gas in question; but the attempt has not even the merit of ingenuity; the very essay itself furnishing incontrovertible arguments against the supposed identity. "According to Dr. Beddoes (says he) the blood and muscles of animals killed by confinement in this gas, have a red colour, less distinct than that produced by oxygen, but very different from the dark colour exhibited by animals killed by drowning or carbonic acid." Now it is a notorious fact, that the blood of those exposed to miasmatic exhalations, instead of being redder, is darker than natural. "It is a matter of common observation, (says Cooke) that the blood drawn from persons who have been exposed to the exhalations arising from marshes, is darker than usual. It is often nearly black, even in those persons who are not ill, the darkness being in proportion to the degree of exposure."† It is still more unfortunate for their identity, that the Doctor should have mentioned several instances in which this very gas was applied in a very concentrated state to the system, without even the production of an intermittent fever. One instance is that of Sir Humphrey Davy, who took three inspirations of the pure gas. This perilous experiment was near depriving science of one of her favourite votaries, not, however, by means of fever; "for, (says he) I slept sound, and awoke in the morning very feeble and hungry. If the carburetted hydrogen be Malaria, and Malaria be capable, as some contend, of producing the plague, we think that Sir Humphrey Davy ought certainly to have fallen a victim to it.

He also mentions the case of Mr. Greenwood, who breathed four quarts of the air diluted, and though the effects were highly distressing, nothing like fever followed.

"A person standing in a current of air, while about a cubic foot of the gas was set free, felt as it passed him, very sensible depression with vertigo." No fever resulted. "Another person was thrown into a state of complete syncope, by standing near a tube from which the gas issued for a short time," still no fever. If the Doctor's object had been to disprove instead of to prove their identity, he could not have

* American Jour. vol. xi. p. 38. † Treatise on Pathol. and Therap. vol. i. p. 285.

furnished *stronger evidence* against it, than *he* has exhibited in its favour.

Dr. William Currie, of Philadelphia, thought "that the unwholesomeness of low and moist situations in the summer and autumnal months, is not owing to any invisible miasmata, or noxious effluvia which issue from the soil and lurk in the air, but to a deficiency of the oxygenous portion of the atmosphere in such situations, in consequence of vegetable and animal putrefaction, in conjunction with the exhausting and debilitating heat of the days, and the sedative powers of the cold and damp air of the nights. "But, as Bancroft observes, if the mere abstraction of a part of the ordinary proportion of oxygen in the atmosphere could occasion intermittents and remittents, those fevers ought to be produced in every crowded assembly, and in a multitude of situations where no such effect has been observed or suspected.*"

We have thus presented a few of the conjectures, which have been advanced as to the nature of Malaria. None of them will bear the test of examination, and consequently its discovery still remains a desideratum. We have not even a conjecture of our own to offer.

3d. *What are the sources of Malaria?* The most prolific sources of miasma, are unquestionably, swamps and marshes; hence the common term Marsh Miasma. But though the diseases arising from Malaria, prevail, most frequently and extensively in the neighbourhood of swamps, &c., they are by no means confined to them. Wherever they do appear, however, we shall find something equivalent to a marsh. "Mud and stagnant water, (says Jackson) in every climate possess the materials of this species of disease," hence fevers are so often distinctly traceable to the exhalations arising from the slimy and half dried margins of pools of stagnant water, or of mill ponds, or the exposed banks or bottoms of rivers; and sometimes also to large heaps of decaying vegetable matter. But in whatever situation this gas is exhaled, whether from a marsh strictly so called, or from the exposed margins of ponds, rivers, &c., or from heaps of vegetable matter, we shall always find the co-operation of several circumstances necessary; without which it is not produced; these are heat, moisture, vegetable matter and the contact of air. Neither of these agents can separately generate miasmata, nor can any two or even three of them, without the concurrence of the fourth. From the repeated observations of this fact, physicians were led to suppose that Malaria was the result of a putrefactive process, the same circumstances being requisite to both. The co-existence of those causes which promote putrefaction in either vegetable or animal matter, with those which give rise to fevers, are uniform and

* On Fevers, p. 145.

invariable. Fevers like putrefaction, only prevail to any extent in the hot season of the year; and as this process is most active and intense, in the hottest countries, so are fevers there most violent and fatal; cool weather which checks or mitigates putrefaction, renders much less numerous and milder the cases of fever; and severe cold which entirely suspends it, is followed by a complete disappearance of disease. Excessive dryness and excess of moisture, both equally unfriendly to putrefaction also procure an exemption from epidemics.

We have said above that marshes are the most prolific sources of disease. Wherever these exist to any extent we shall invariably find them under certain circumstances exhaling this pestiferous gas.—Those countries where sickness prevails throughout the year, as Benin, New and Old Calabar, &c., are low and marshy. Those cities and stations in Africa, Asia, and America, which have been the graves of unnumbered thousands, are situated near marshes, or at the mouths, or on the banks of rivers in flat countries, which in the rainy seasons become one vast morrass.” In the East they prove almost the only source of disease; the wide spread marshy surface left by the retiring inundations of the Ganges, “exposed to the rays of a vertical sun, that has lost nothing of its power by a Southern declination,” exhales in profusion this morbid principle, which “spreads pestilence and death in every direction.” In Egypt we find fevers appearing as soon as the country is brought into a marshy state, by the drying up of the inundations of the Nile.

If we look to the West Indies, we shall find the same cause of disease, operating to a great extent; those towns and situations in the neighbourhood of marshes alone being sickly. In more temperate climates we shall find the same effect from marshes. Italy affords a melancholy instance of their dire effect. The ancient *Latium* with her hundred cities, the present *Campagna di Roma*, has been rendered almost a desert by the miasmatic exhalations which are there generated by almost every foot of the soil. The extensive marshes of Holland are notoriously the cause, of the terrible epidemics which have so frequently ravaged that country. Hungary has also suffered extremely from the same source.

In our own country, we shall find the rice swamps of the Southern States and the swampy countries of the sea coast from Delaware bay southward, to prove fruitful sources of disease. The annual inundations of the Mississippi, leave a vast extent of country in a marshy state, from which miasma in abundance proceed.

Independently of the strong presumptive evidence of the unhealthiness of marshes, afforded by the almost invariable occurrence of disease in their neighbourhood, we have positive proof to the same effect. Thus a place has been rendered suddenly unhealthy, by the

shifting of the wind, so as to blow the atmosphere of marshes upon it. Lind, speaking of a settlement formed by the English, on the island of Balambangan, near Borneo, says, for a few months these people continued perfectly healthy ; but no sooner did the monsoon change, than sickness made its appearance ; and it raged with such violence that scarce one in ten survived this monsoon. "The seasons of health and sickness are here regulated by the direction of the wind or monsoon ; from October until April, during the northeast monsoon, the wind comes from the sea, and the settlement is perfectly healthy ; but from April, until October, during the southeast monsoon, the wind blows over the marshes, both of this Island and of Borneo, and produces fevers of a most malignant nature, which frequently cut off even the stoutest men in twelve or fourteen hours."*

Lancisus relates that thirty gentlemen and ladies of the first rank in Rome, having made an excursion upon a party of pleasure towards the mouth of the Tiber, the wind suddenly shifted, and blew from the South over the putrid marshes, when twenty-nine were immediately seized with a tertian fever, one only escaped."†

The following account of the fatal fever which prevailed at Gallipolis, on the Ohio river, in the summer of 1796, was furnished by Major Prior of the army, to Dr. Potter of Baltimore, who inserted it in his memoir on contagion. "As the garrison was sorely afflicted (says Major Prior,) by this fever, I could but take great interest in it. The fever, was, I think, justly charged to a large pond, near the Cantonment. An attempt had been made, two or three years before, to fill it up by felling a number of large trees, that grew on, or near its margin, and by covering the wood thus fallen with earth. This intention had not been fulfilled. In August the weather was extremely hot and uncommonly dry ; the water had evaporated considerably, leaving a great quantity of muddy water, with a thick slimy mixture of putrefying vegetables, which emitted a stench almost intolerable. The inhabitants of the village, principally French, and very poor and filthy in their mode of living, began to suffer first, and died so rapidly that a general consternation seized the whole settlement. The garrison continued healthy for some days, and we began to console ourselves with the hope that we should escape altogether : we were however, soon undeceived, and the reason of our exemption heretofore was soon discovered. The wind had blown the air arising from the pond, from the camp ; but as soon as it shifted to the reverse point, the soldiers began to sicken ; in five days half of the garrison was on the sick list, and in ten half of them were dead."

In the year 1794, a fever broke out at Rockfort, in France, which

*Lind on hot climates, p. 74.

†Ibid, p. 21.

on account of the malignant symptoms and great mortality, was at first believed to be the true plague. But M. Chirac, who was sent by the court to inquire into its nature, found the cause to arise from some marshes that had been made by an inundation of the sea, and observed that the corrupted steams, which smelled like gunpowder, were carried to the town by the wind that had blown long from that quarter.”* We have then positive evidence of the morbid principle contained in the air of marshes.

We have now arrived at the much vexed question, of whether Malaria proceeds equally from vegetable and animal putrefaction? or is the exclusive product of the first? To the first of these questions, we would answer in the negative, to the last in the affirmative. In discussing this subject, we will, in the first place, endeavour to show from the numerous facts on record, that Malaria is produced by vegetable matter *per se*, in a state of putrefaction. 2d. That animal putrefaction is not a source of Malaria, and therefore is never the cause of epidemic disease.

1st. *Malaria is produced by vegetable matter per se in a state of putrefaction.* Fevers which appear in vessels at sea, afford, perhaps, the most unexceptionable proofs of the morbid principle generated by vegetable putrefaction. “The holds of all vessels, (says Bancroft) particularly ships of war, contain large quantities of vegetable matter in casks, often insufficiently strong to prevent their contents from escaping; such as peas, oatmeal, cocoa, flour, sugar, and wood for fuel, and when united with a due proportion of humidity, so as to produce a given change by putrefaction, a gas is evolved under certain circumstances of temperature, highly detrimental to animal life.”†

Many instances of most malignant fever arising under such circumstances might be mentioned. “The ship Antelope arrived at Barbadoes in March. Immediately afterwards, when the inhabitants of Bridgetown were perfectly healthy, a fever appeared on board; there were one hundred and ten cases, of which thirty-one ended in death. In consequence of this, she was cleaned out, when it was found that a “combination of the above vegetable substances,” with the bark of green-wood and chips in a state of putrefaction, occupied a considerable part of the hold; and those individuals who had hitherto resisted the influence of this miasma, now suffered in clearing away a mass of highly putrid and offensive matter.”‡

The ship Regalia, left Guernsey in November, 1814, and arrived on the coast of Africa in February, 1815, full of troops in perfect health. She was employed from that time until the latter end of June

* Pringle on Diseases of the army, p. 323. † On Fever, p. 233.

‡ Bancroft on Fever, p. 233, as quoted by Cooke.

in removing troops and stores from one port to another. At this time she took in a supply of green wood, which was "cut down and brought on board the same day," in such quantity that after she had been in the West Indies several weeks, there was enough left for a voyage to Europe. The ship was rather leaky.

She sailed from Africa with black troops for Barbadoes on the 18th of July, and arrived a little before the 24th of August. There was a considerable mortality on the passage. "At English harbour, Antigua, she was fumigated, and after three days detention, returned to Barbadoes. During this short voyage, three newly entered hands sickened; one died, and two were sent to the hospital; on her arrival three more sickened, one of whom had lately entered, and one died."

"The hold was now ordered to be cleared out; every thing was taken out, and the hold was exposed to the concentrated heat of stoves, the hatchways being closed. About the time this was commenced, of three men, newly shipped, two sickened, of whom one died: and the cook, upwards of fifty years of age, sickened and died."

"After having undergone the above thorough cleansing, she sailed from Guadaloupe, crowded to a very great degree with French prisoners and their families, from the jails, under the most dangerous circumstances to health, with a case of yellow fever actually dying on board the day before she left Basse-Terre roads, but without communicating any such to the unfortunate passengers."*

The two following instances of fever of the most malignant type generated on ship-board, by the putrefaction of vegetable matter, are very striking in their character. They are from the official reports of Dr. Hartle, an English navy surgeon: "On the 31st of August last, (1821) his Majesty's armed transport *Dasher*, left Carlisle Bay for Barbadoes, bound for Tobago; but in consequence of severe gales of wind, she was obliged to go to St. Lucia. From thence she proceeded to Tobago, and received on board a company of the 9th Regiment, and conveyed them to Grenada, that they might avoid the endemic fever of the former place." * * * * The *Dasher*, after landing this company at Grenada, proceeded hither, (English Harbour,) but a few days before she reached this port, yellow fever made its appearance among her crew, and previous to her arrival, six men had been attacked, two of whom died. In the evening of the 22d of September, she arrived in English Harbour, and on the 23rd, the assistant surgeon, Mr. Maclean, reported two men ill of yellow fever. One died in a few hours. On the following day, (24th) seven men were attacked through the day, all suddenly and seriously." I immediately addressed his excellency Major General, Sir B. D'Ur-

* Cooke's Pathology and Therapeutics, vol. i. p. 85.

ban, reporting the circumstances, and recommending that, without loss of time, the officers, crew, and marines be ordered out of the ship, and encamped on an isthmus of land at the north of the harbour: and that black people he employed to take out all her stores, tanks, ballast, &c." This was accordingly done on the 26th September: "Black people immediately commenced cleaning out the ship: after removing the stores, tanks, fuel, ballast, &c.; the dirt was scraped together, but this amounted to no great quantity. This did not satisfy my mind, and I insisted upon having the limber boards taken up. Here, to the astonishment of every one, lay the mischief. On taking up these boards the noxious effluvia surpassed any thing that I had before experienced, and it was with difficulty that the blacks, who were accustomed to this work, could remain. Between the timbers there was a collection of carpenter's shavings, &c. in great quantities. These had so completely choked up the limber holes, that the water could not pass to the well of the pumps, and lay stagnant. The vegetable matter, was therefore, in a state of decomposition, and this acted on by high atmospheric temperature, became neither more nor less than a marsh in the centre of the ship."*

"His Majesty's ship *Pyramus* left this port, (English Harbour,) as I understood, perfectly healthy, on the 19th October, in company with the flag ship of rear admiral Tackni, for St. Kitts, where she remained until the 28th of October, when she sailed from thence for this island. A day or two prior to her arrival here, fever of a most alarming type made its appearance among the officers and crew. She came into English Harbour early in the morning of the 1st of November, and as soon as she was anchored, Capt. Newcombe sent a lieutenant to inform me of the state of the ship, and to request medical assistance. I immediately went on board, and was surprised to find that an officer (lieutenant) had died the day before, with only a few hours illness—that the purser and six men lay dangerously ill; the surgeon, who from his own account had been attacked himself, was unable to perform any duty." After landing the sick, "the *Pyramus* proceeded in the afternoon of the 2d of November to Barbadoes. Mr. Freer, who returned to this garrison on the 9th instant, having been relieved by a naval surgeon, reports that many fresh cases had occurred in the passage up, but that he had landed most of them at Barbadoes in a convalescent state; that when he left the ship the fever still prevailed."

The fever continued to prevail on board this vessel, and finally become so alarming that a board of medical officers was ordered, at the island of Barbadoes, to "investigate the probable cause of the sick-

* Med. Chi. Review, March, 1824, p. 994

ness that prevails on board." The board of medical officers recommended as a means of health, that the *Pyramus* should be taken out to sea; accordingly Capt. Newcombe left Carlisle Bay, Barbadoes, and cruised as far as 28° north; "but, finding this avail nothing, and that the disease became more alarming, he hastened to English Harbour, and arrived here on the 3d of January, 1832. I anxiously embraced the opportunity (says Dr. Hartle) to ascertain if my prediction (viz: that the source of the sickness was in her hold,) was correct, and immediately visited the ship. On finding that the sickness continued with increased violence, I requested permission to examine the vessel, which I did in the most minute manner; and it is but truth when I say, that no ship could possibly appear cleaner or in better order;—and had I not been convinced that it was in her hold, I should have been at a loss, from the good order and apparent comfort of the people, to account for the cause of so malignant a fever. I instantly removed all the sick to the hospitals, and wrote to Captain Newcombe, recommending the removal, as soon as possible, of all the officers and crew from the ship, and that black people should be employed to do the laborious work of removing her stores, shot, tanks, ballast, &c. and cleanse (under my frequent inspection) the holds and every part of the vessel." The recommendation of Dr. Hartle was acquiesced in; and the officers and crew were landed on the 15th. Anxious to ascertain, by ocular demonstration, the cause of this endemic, I daily visited the ship, and was present at the opening of the holds—the effluvia from which surpassed any thing I had before experienced. Zeal on the part of the first and second lieutenant, master and surgeon, incited them to accompany me, and they were accordingly with me when the limber boards were taken up. It is not in my power to describe the stench which issued forth when they were raised. The boatswain, curious to see what we were doing, did not come down, but lay on the lower deck to view us. This confined gas rushed up the hatchway, where his head was hanging over, so oppressively offensive that he fainted. In the night he was attacked by the fever, and narrowly escaped. On the following day the first lieutenant was attacked. In the evening the second lieutenant was taken ill, and in a few hours after the master and surgeon. On the third day I was myself very much indisposed, but fortunately diarrhœa removed all my febrile sensations." "Although (continues he) I was fully persuaded, in my own mind, that the cause of the disease lay in the ship's bottom, I had not an idea, that such extensive bogs could have been found under the limber boards, and between the timbers. It would appear scarcely credible, that four large mud boats of filth should be taken out of this frigate, which had been only six months from England, and I believe not long out of dock. This, however, is easily explained:

The ship-wrights had never taken out their chips and shavings—the coal tar, from heat of the climate, had run into all the limber holes, and so cemented those chips and shavings, that no passage remained for the water to go to the well of the pumps; it, therefore, lay stagnant under a heat of 100° of Fahrenheit's thermometer."*

The steeping of hemp and flax, in stagnant shallow water, ("*ubi palustres disident aquæ, ventique silent*") in consequence of which it has putrefied, has also been productive of disease. Lancisi "gives the history (says Bancroft) of an epidemic commonly intermittent, and often resembling the tertiana lethargica of *Forti*, which for several summers infested, and almost depopulated the ancient town of Urbs Vetus, in an elevated and salubrious part of Etruria; and which was occasioned by ponds or stagnant waters in the lower part of the town, in which hemp and flax were macerated: (*in quibus linum, et cannabis macerabantur*,) but this being prohibited in 1705, the fevers did not afterwards occur."†

Fevers have also been known to arise from the putrefaction of "the vast heaps of the indigo plant, which are negligently formed, (after the colouring principle has been extracted) near the works and houses of the labourers, and there left to decompose and become manure, which is of an excellent quality, after two or three years. These heaps wetted from time to time by heavy rains, and afterwards heated by the powerful rays of a vertical sun, emit, very copiously, vapour, or miasmata, resembling in their effects those of marshes, for those persons who live near to, and especially on the leeward side of these fermenting vegetable masses, are commonly attacked by fevers, chiefly remittents, and similar to those which prevail in marshy situations.—And according to my information (continues Bancroft) the connexion of these fevers with the heaps of fermenting indigo plants is now so well understood and believed in that part of the world, (East Indies,) that the more intelligent indigo makers no longer permit such heaps to be formed near their works, or the habitations of the workmen, but cause them to be placed at considerable distances, and to the leeward thereof, and thus preserve their labourers in health."‡ Many other instances might be mentioned in which Malaria was produced by vegetable putrefaction, but those cited are deemed sufficient to prove the position with which we set out.

2d. *That animal putrefaction is not a source of Malaria, and therefore is never productive of epidemic disease:*

We had supposed that the opinion of the harmless character of the exhalation arising from animal putrefaction, at least so far as relates to

* Med. Chi. Review, March, 1824, p. 997-8.

† On Fever, page 1, 146 note.

‡ Ibid.

the production of "epidemic evils," was so well established from the numerous facts which have been brought to sustain it, as to defy all attempts to contradict or impugn it. A late writer, however, (Dr. Parsons) has endeavored to disarm the facts alluded to of all their force, and by many other facts, to prove that epidemics have and may arise from animal putrefaction.

His first attack is upon the history of the exhumation at the church yard of the St. Innocens, at Paris, contained in D. Bancroft's work on fever. This place "had been made the depository of so many bodies, that, although its area enclosed more than one thousand seven hundred square toises, or near two acres, yet the soil had been raised by them eight or ten feet higher than the level of the adjoining streets; and upon the most moderate calculation, considerably more than six hundred thousand bodies had been buried in it, during the last six centuries, previous to which date it was already a very ancient burying ground. Numerous complaints having been made concerning the offensive smells, which arose from this spot, and sometimes penetrated into the adjoining houses; and the public mind being greatly alarmed, it was at last determined to forbid all future burials there, and to remove so much of the superstratum as would reduce the surface to the level of the streets. This work was undertaken in 1786, under the superintendence of M. Thouret, a physician of eminence in Paris, and in two years he accomplished the removal of that superstratum, almost the whole of which was impregnated with the remains of carcases, and of quantities of filth and ordure, thrown upon it from the adjoining houses." "The exhumations were principally executed in the winter, but a considerable part of them was also carried on during the greatest heats of summer."

"They were begun with every possible care, and with every known precaution; but they were afterwards continued, almost for the *whole* of the operation, without employing, it may be said, *any precaution whatever*; yet no danger manifested itself during the whole course of our labours—no accident occurred to disturb the public tranquillity."

"In referring to these and all other cases of animal decomposition, (says Parsons) we are to bear in mind the circumstances we have already related, of vegetable matter, as necessary to constitute it a cause of malignant, yellow, or putrid fever. And in the first place, in respect to heat. This it has been shown must range above 60° of Fahrenheit, for several days, in order to generate these fevers from vegetable decomposition. Now what was the temperature at the time the exhumations took place? They were commenced at Paris, in December, 1785, and continued till May, 1786—renewed again in the following December, and continued till February, and from the month of August, 1787, to the month of October. There were then only

one or two months of the time in which malignant, putrid or yellow fevers, from even vegetable putrefaction, or any other cause are supposed to occur."

He, (Bancroft) is cautiously silent, (says Parsons) as to the time of the last interments, and also as to the time that animal matter is supposed to retain its febrific qualities. Now what is the fact in respect to these particulars? In respect to the duration of this morbid principle in decomposing animal bodies, Maret, an eminent French writer, states three years as the time for complete decomposition, when the grave is four feet deep, and four years when it is six or seven." "What says Fourcroy? This gentleman, who was employed for chemical purposes in the exhumation of St. Innocens, states, three years as the time during which the septic poison must take place."

"Speaking of this very exhumation, he remarks, "we had a strong desire to satisfy ourselves by experiment, what was the nature of the destructive air or septic principle, emitted from corrupting bodies, but we had no opportunity, in consequence of there having been no burials there for the three preceding years; the last deposit there being in 1782." "Now, the month of August, 1787, (says Parsons) was the time when the bodies were removed in the hottest weather, as Bancroft relates it, which was five years after the last interment, and two years after morbid miasm, according to Maret and Fomeroy, had ceased to exist. Nor is it at all likely that the last interments five years previous to said August, were especially reserved for this hot month, but most probably much older interments. There were, therefore, no bodies removed that were in a stage of decomposition favourable for producing febrile diseases, in *any* weather, however hot, even had the whole number been exposed to the air of mid-day at once."*

Notwithstanding what Dr. Parsons has said in the above lengthy extract, against the validity of this extensive and unparalleled exhumation, as proving the innoxious nature of the gases arising from animal putrefaction, we are still of opinion that it is as strong a case as could possibly be brought forward to support the doctrine. The Doctor certainly has not weakened our faith in it by his laboured criticism; for he has not advanced a solitary argument against it. His chief objection is based upon the *mere opinion* of two distinguished men, (one a chemist of celebrity,) that the septic principle ceases to be generated after three years' interment, and consequently that this removal of thousands of dead bodies was unproductive of disease, because, forsooth, there had been "no burials there for the preceding three years." Now even granting this opinion to be true, to give the opinion any force, it must be proved that this septic principle is the morbid ingredient, (if there be any) in the results of animal decomposition, which certainly cannot be done.

*Med. Chi. Review, April, 1825, p. 406.

Fourcroy confesses his ignorance of the nature of this septic principle, as he terms it ; it had a mere ideal existence, and yet he speaks of it with as much certainty as if it was both visible and tangible. If it is not generated after three years' interment, pray, at what period is it generated ? If Fourcroy knew with so much certainty that it was not produced after three years' interment, he must have known with equal certainty that it was generated during the period extending from the time of burial to the end of the three years, else he could not have assigned so exact a limit to its production. And how could he arrive at this knowledge ? by *one only* of two ways ; either by subjecting it to chemical analysis, and thus first ascertaining its existence, and afterwards by repeated analysis of the exhalations from animal decomposition within the time specified, he might learn when it ceased to be generated, or he might judge of its presence or absence by its effects upon the human constitution. That he did not acquire the knowledge in the way first mentioned, is obvious, because he remarks, "we had a strong desire to satisfy ourselves by experiment what was the nature of the destructive air or septic principle," and if he judged of its existence, and the duration of that existence, by its effects, we would reply that those very effects are the subject matter of dispute, and we would ask, if it is probable that Fourcroy ever had the opportunity to observe a single example of animal decomposition for three successive years, so as to tell when this septic or morbid principle was and at what time it ceased to be generated. The fact is, it is a mere conjecture, and one too fraught with absurdity ; and even were this not the case, it is by far too slender a foundation to build an argument upon for the overthrow of an important fact.

It is perfectly obvious to common sense and reason, that so long as there is matter to undergo decomposition, that Malaria, (if it be a product of this decomposition) must be exhaled ; and that there was an abundance of putrefying materials is evident from the fact, that almost the whole of the removed superstratum "was impregnated or infested, as M. Thouret styles it, with the remains of carcasses." And again, "grave-diggers were thrown down suddenly and deprived of sense and motion, on breaking open with their spades, the abdominal viscera of corpses."* Here we have the extrication of a gas, so concentrated as to produce asphyxia, and yet we are to be told that fever was not produced, because the septic principle was wanting.

We would ask Dr. Parsons, (who cannot consistently deny that Malaria in abundance must have been generated at some period during the progressive decomposition of this immense collection of dead bodies,) what became of the immense volume of it which must have

* American Journal, No. 3, p. 98.

been annually disengaged as long as it was used as a burying ground? It must either have escaped through the cracks in the superincumbent earth, or it must have lurked in the soil. That it did not do the first, will be admitted by the advocates for the febrific qualities of animal miasm; because the consequence must have been an annual and destructive epidemic in the neighbourhood; and no such thing has been contended for. It certainly at least did not constitute one of the reasons for so vast an undertaking, "the offensive smells which arose from this spot, and sometimes penetrated into the adjoining houses," being the only reason mentioned for removing the superstratum, and that it did not remain in the earth we may infer from its not producing a single case of fever during the exhumation. The truth is, it had no existence; for if Malaria be a product of animal decomposition, the whole neighbourhood of the St. Innocens would have been annually devastated by fever. The *offensive* gas or gases of animal decomposition, escaped from the soil and pervaded the whole neighbourhood; and if there be a septic principle or morbid gas distinct from the former, we see no reason why it also should not have escaped from the superjacent earth.

"Another fact particularly worthy of notice (says Parsons) which Bancroft ignorantly or unfairly omits to mention is"

"The great number of torches and fires that lighted all parts of the cemetery, and shed around a melancholy glare," and "the thick clouds of smoke that surrounded and covered the place of labor." We cannot subscribe to the charge either of unfairness or ignorance in Dr. Bancroft, in failing to mention the above circumstance; a much more charitable supposition would be, that he might have considered it perfectly immaterial and unimportant.

Dr. Parsons next notices the mission of M. Berthe, Professor in the school of medicine of Montpellier, who with "two of his colleagues in that University, were sent by the Government of France, into Spain, to examine and report upon the nature of the Yellow fever, which had proved so fatal in several towns in Andalusia, in 1800." M. Berthe, states that he was at Seville in the spring of the year succeeding the epidemic, and "frequently visited the burying places just without the city, in which the victims of the fever had been interred." "Many thousands of the inhabitants of Seville, also, came hither, says he, some for curiosity, and others in processions, to testify their sorrow and respect for their departed friends. In one of these grounds, southwestward of the city, ten thousand bodies had been buried; in two others, seven or eight thousand; and in that of Triana, about four thousand."

"The heats of spring, (says M. Berthe,) which I need not observe are considerable at Seville, were at this time beginning to be felt, and, the ground of these burial places being clayey was already cracked

into wide and deep crevices, through which a fœtor was exhaled, the result of the decomposition which was going on among these heaps of bodies.”

From Seville the commission went to Cadiz “where they found the churches filled with putrid emanations from the same cause,” but as they did not discover that these supposed fomites of infection were productive of any mischief, their fears concerning them seem at length to have completely subsided; for in their reply to the President and members of the board of health, who had requested a statement of their opinion, they expressly declare their belief, that if the Yellow fever could be produced by the effluvia arising from putrefying bodies, it was evident that such a misfortune must have already taken place, through the imperfect manner in which the tombs and vaults, pointed out by them had been closed; a defect which they had observed even in the churches most frequented.” “Thus it appears (says Bancroft,) that the putrid emanations from the bodies of many thousand persons who had recently died of Yellow fever, did not, and therefore could not produce that disorder.”

To the preceding facts (says Bancroft) I may add another, which is related by a man whose veracity is as little to be questioned as his exalted philanthropy—I mean John Howard, in his work on *Lazarreto's*, page 25.” “The Governor of the French Hospital at Smyrna told me (says Mr. Howard,) that in the last dreadful plague there, his house was rendered almost intolerable, by an offensive scent, especially if he opened any of the windows which looked toward the great burying ground where numbers were left every day unburied; but that it had no effect on the health of himself or his family. An opulent merchant in this city, (adds he) likewise told me that he and his family had felt the same inconvenience without any bad consequences.”

Dr. Parsons objects to the conclusions arrived at by M. Berthe, and his colleagues of the incapability of animal decomposition to produce Yellow fever, upon the ground that the heat of the season was insufficient to generate the febrific miasmata, which he alleges to arise from that source. “And who, let me ask, (says he) will pretend that the thermometer ranges at Seville, so high as 80° in the spring, for a succession of days, or that Yellow fever ever prevails there till late in the summer.”*

Now granting to Dr. Parsons, the full force of his objection to this statement, upon the ground of insufficient temperature, at the time alluded to, namely, the spring of the year. Yet the fact that the putrefaction of thirty thousand bodies, about the total number of deaths in

* We purposely omit copying the personal remarks of Dr. Parsons upon Dr. Bancroft; they certainly would be out of place here, and Dr. Bancroft, if alive, is fully competent to defend himself from all such aspersions.

Cadiz and Seville, in the summer and fall of 1800, did not produce another wide-spread and desolating pestilence, during the excessive heat of the succeeding summer, when they must have been in every stage and degree of decomposition, is proof positive and irrefragible, that the putrid emanations from decomposing animal matter is incapable of producing "epidemic disease." M. Berthe, and his colleagues were at Seville, in the spring of the year 1801. It is stated by him that the "earth was already cracked into wide and deep crevices," and if the comparatively mild temperature of the spring had already "cracked the earth into wide and deep crevices," it is obvious that the great heat of summer, must have very much increased and widened them. These cracks had no doubt a direct communication with the putrefying bodies—for M. Berthe mentions the fœtor of the decomposing bodies escaping through them.

At Cadiz we have even a worse state of things, all the churches, &c., being filled with the stench by the putrefying bodies in the tombs and vaults, which were very imperfectly closed; this condition of things too, must have been very much aggravated by the heat of the succeeding summer. In these instances then, we have thirty thousand bodies in a rapid state of decomposition, and in a situation too almost equivalent to that of laying on the surface of the earth, without the production of epidemic disease in their neighbourhood.

As to the statement of Mr. Howard, Dr. Parsons hardly honours it with a passing notice. The febrific principle (says he) was probably in a great measure excluded by keeping the windows closed. And again, this story comes third handed, and without any statement of the distance of the burying yard from the houses, or the temperature of the air.

As to the manner in which this story, as he terms it, comes to us, we conceive it to be perfectly unexceptionable, and entitled to full and implicit belief. It was made by the Governor of the Hospital at Smyrna to Mr. Howard, who considered it so remarkable, as to deserve publication in his work on *Lazarretto's*. Neither of them had any prejudice to combat, or theory to sustain; it was stated as a simple fact and so recorded, and if it is of any importance to correct the Doctor in so trivial a matter, we would remark that it comes second, and not third handed, as he states it. As to the distance of the burying ground from the houses and the temperature of the air, the words "almost intolerable by an offensive scent," certainly implies that the one could not have been far, and that the other must have been high; the scent could not be intolerable, without considerable putrefaction in the exposed bodies, and putrefaction could not take place to any extent, without the presence of a high degree of heat. But besides this, if Dr. Parsons will consult the tables on the subject, he will find that the plague generally begins, increases, and declines, at the

same period that marsh fevers do, and that consequently this affords an additional reason for supposing that the weather must have been hot.

In this instance then we have a striking proof of the innoxious nature of animal miasma; "numbers were left every day unburied," and in that hot climate too, where putrefaction must have been very rapid and intense, and yet no ill consequences resulted from it.

"In the appendix of Bancroft, (says Parsons) is a long account of the innoxious effects of dissections. But are these, (says he) ever prosecuted in the heats of summer, when yellow fever prevails? To this question we would reply in the affirmative. Dr. Cooke says, "I have known some students have in the very heart of a town, in the chief place of concourse, a number of dead bodies in the midst of summer, in the most offensive state, so that great complaint was made of the nuisance, without producing any ill effect whatever to themselves or others, except some little nausea and perhaps head-ache in one of them."*

"The account of an adipocise establishment near Bristol, in England, is unaccompanied by any statement of the season of the year, or of the temperature of the atmosphere, or the number of persons exposed to the effluvia, or the amount of putrid animal matter existing at the same time. The same imperfection attends the accounts of the dead bodies washed on shore near Aboukir; the glue, soap, candle catgut and leather factories."

It will be observed from the above, that Dr. Parsons dismisses these cases illustrating the harmless character of animal miasmata, in a very summary and expeditious manner. But he will not be suffered thus to dispose of them without some better and more substantial reasons than he has given in the above extract, not one of which we venture to say can be sustained.

We will first give the accounts entire, as detailed by Bancroft, and then make our remarks upon them.

"In the Edinburgh Medical and Surgical Journal, for October 1st, 1810, may be seen an account given by Dr. Chisholm, of a manufactory, (of which I had some knowledge from the time of its first establishment) at Conham, near Bristol, destined for the conversion of animal flesh into a substance resembling spermaceti, by cutting up dead horses, asses, dogs, &c., and putting their muscular parts into boxes with holes for the admission of water, and afterwards placing them in pits filled with water, while the entrails and useless parts of many hundred carcasses were left to putrefy on the surface of the earth. And it appears from Dr. Chisholm's statement, as well as from other infor-

*Treaties on Pathology and Therapeutics, vol. 1, p 150.

mation which was given to me on the subject, that though the effluvia from these putrefying animal matters was highly offensive to the overseer of this Manufactory, and to the workmen employed under him, as well as to others within their reach, no injury was done by them to the health of any person, during the two years in which these operations were continued."

"During the time that our very numerous fleets of transports lay in the bay of Aboukir, many bodies of sailors who had died, or had been drowned, were washed on the shore, where they remained unburied, exposed to the heat of the sun. In riding to Rosetta it was necessary to keep along the shore; and I passed 18 or 20 corpses in this situation. They were in various states of putrefaction; but the stench from all was offensive in the highest degree, and often extended to more than 100 yards. My curiosity led me to approach close to most of them, that I might examine the changes they had undergone. Some were swelled up to an enormous size, and the skin seemed so distended, that it appeared ready to burst. These were often of a dark colour; some had not yet come to that state, others had passed it; and the skin having burst in several places, the air had escaped, and they had become more or less desiccated and of a black colour. Every person who had occasion to pass from the camp to Rosetta, was obliged to come within reach of the vapours emitted by these bodies. There were orderly dragoons constantly passing; yet, neither myself or any one else was attacked with fever, in consequence of our exposure to these vapours; and my professional situation would probably have enabled me to learn if any such occurrence had followed."

Now we admit that in the statement of Dr. Chisholm about the Manufactory at Conham, it is not said in so many words, that the weather was very hot, or that a given quantity, or what quantity of animal matter was exposed on the surface of the ground at one time, but from the expression "the entrails and useless parts of many hundred carcasses were left to putrefy on the surface of the ground, it is obvious that it must have been considerable; and that it was in a state of putrefaction, is equally evident from the expression, that "the effluvia of this putrefying animal matter was highly offensive to the overseer of this Manufactory," &c. It is mentioned, too, that these operations were continued during two years, from which we must conclude that they were carried on in the summer and autumn of these two years, as well as during the other seasons. A part then, of these remains "of many hundred carcasses," must have been exposed to the hot sun of these two seasons during two successive years, which surely must have been sufficient to have exhaled even the septic principle, which Dr. Parsons identifies with the noxious miasm (if there be any) of animal putrefaction; and yet we are told that not the slightest febrile indisposition was induced during the whole of this time.

As to the occurrence in Aboukir bay, we would observe that the bodies "were in various states of putrefaction," which certainly implies that there was a sufficient degree of heat to bring them into that state : and we may also presume there was from the expression "where they remained unburied, exposed to the *heat of the sun*."*

Bancroft mentions an establishment at Greenland dock for boiling blubber. "Mr. Ritchie, (says he) had lived more than fifty years in the neighbourhood of this dock, was well acquainted with the boiling process, and assured me repeatedly, that though the blubber is often in a very offensive state, emitting an highly putrid smell, neither himself, nor his people, nor the crews of the Greenland ships, who perform the whole boiling, &c., nor the neighbours, have ever to his knowledge suffered in their healths from that operation." "The stench from the blubber is universally admitted to be greatest when it is boiling."

In noticing this fact, Dr. Parsons remarks, "that the boiling of blubber should be harmless notwithstanding its offensive effluvia, is what certainly might be expected, considering the great power of fire as before stated on several authorities to destroy Malaria."

The heat of fire, like that of sun, rarifies and disperses Malaria. It is not supposed by any so far as we know, that any thing like decomposition or destruction of this gas takes place. Now if it takes an upward course under the operation of heat, the circumstance mentioned in the latter part of the statement under now consideration, and which Dr. Parsons either overlooked, or disengenuously omits to mention, viz., that the effluvia arising from the boiling blubber, ("so far from being unhealthy, are, on the contrary reckoned so wholesome that it is very common for sick persons to come to the copper, as soon as they arise from their beds, and to hold their heads over the steam, as close as they can") is perfectly conclusive against the supposition of the escape of the Malaria upwards, during the process of boiling the putrid blubber, and that so far from being destroyed by fire, there is none to destroy.

The last instance from Bancroft, mentioned by Dr. Parsons, is the putrefaction of fish. "The use of fish for *manure*, such as herrings, alewives, &c., is adverted to, says Parsons, by Bancroft, and also by the gentlemen before alluded to, in Boston and Philadelphia. It should be borne in mind, however, (continues he) that most of these fish are buried, and that such of them as are above ground, are strewed over it, and from their small size are soon dried."

The highly fertilizing qualities of fish manure, arises entirely from

* We hope Dr. Parsons will pardon us for italicising these words, and not mete us the same measure, he does to Bancroft for using a similar liberty.

their undergoing the process of rotting, or decomposition in the soil, with which they have been slightly covered. Without the taking place of this process, they are of no avail as *manure*, and that it does take place is obvious from the increased productiveness of the land after their application. Now we would ask Dr. Parsons, if even he, would contend, that the covering of two or three inches of loose earth, could form an impenetrable barrier to the escape of Malaria, (if it be generated) into the surrounding atmosphere; and that none escapes is plain from the fact that the neighbourhood remains perfectly healthy after the use of large quantities of fish in this way. It does not escape, because none is generated.

But though most generally spread upon the land, soon after they are caught, this is not always the case; they are sometimes left to putrefy in large heaps upon the shore. "Such is the plenty of fish in our rivers, (says Cooke,) that at most fisheries, great heaps almost every year, putrefy for want of purchasers. But no epidemic fever has ever been known to arise on such occasions."*

We have thus got through the last example of animal putrefaction cited by Bancroft, to show its innocency of producing epidemic disease, which has been criticised by Dr. Parsons, we think his objections to them have been shown to be invalid, and that they still stand the *triumphant refutations* of the theory of the febrific qualities of animal miasmata.

There are some other instances equally strong and convincing with those whose examination we have just finished, upon the negative side of this question, which, as the subject is important, we will bring before the eye of the reader.

Dr. Gordon gives "an account of the putrefaction of 1000 barrels of salted beef, at the island of Santa Cruz, which was finally ordered to be thrown into the sea; and were thus disposed of without having occasioned sickness to any person, in the house, store, or neighbourhood where this putrefaction had taken place and subsisted."† He also gives an account of a whale which putrefied very harmlessly at the same island.

Dr. Merrill, after stating that the epidemic yellow fever which nearly depopulated Pensacola in the year 1822, was "attributed to the importation of damaged cod-fish," says "It is a remarkable fact, that came under my own observation, that the United States troops, stationed in fort Barrancas, consisting of about one hundred persons, were exposed in very close, uncomfortable quarters, almost in the midst of a whole cargo of this putrid fish, which had been wrecked upon the

* Treatise on Pathology and Therapeutics, vol. 1, p. 92.

† Bancroft on Fever, p. 422

beach, and yet not a single case of fever occurred among them during the four weeks they were thus situated, and not until three weeks after the fish had been removed. At the same time a battalion of infantry, that was cantoned one mile distant, in a dry, airy situation, was sorely afflicted with violent bilious fevers. The sailors who had been living on board this fish vessel for a number of months in the West India seas and harbours, and in the midst of putrid exhalations, that were almost suffocating to a stranger, all arrived in Pensacola in good health, and only suffered in common with the other inhabitants, after a residence of three or four weeks in the city.”*

This case needs no comment, it speaks for itself. The concomitant circumstances of the putrefaction of nearly a whole cargo of fish, are such as we presume will satisfy, even Dr. Parsons, as fastidious as he is ; it being very hot weather, and the fish having been wrecked and lying upon the beach, a sufficiency of moisture was undoubtedly present.

“A committee has been engaged in France in examining the circumstances relative to the Knackers operations. His business consist in killing old worn out horses, and in turning every part of their bodies to account. The most singular results which the committee have obtained relate to the innocuous nature of the exhalations arising from the putrefying matters. Every body agreed that they were offensive and disgusting, but none that they were unwholesome : on the contrary, they appeared to conduce to health. All the men, women and children, concerned in works of this kind, had unvarying health, and were remarkably well in appearance and strong in body. The workmen commonly attained an old age, and were generally free from the usual infirmities which accompany it.* Sixty, seventy, and even eighty were common ages. Persons who live close to these places, or go there daily, share these advantages with the workmen. During the time that an epidemic fever was in full force at two neighbouring places, not one of the workmen in the establishment at *Mount Faucon* was affected by it. It did not appear that it was only the men who were habituated to the work that were thus favoured ; for when, from press of business, new workmen were taken in, they did not suffer in health from the exhalations.”

“In confirmation of the above observations, similar cases are quoted. Above two hundred exhumations are made yearly at Paris, about three or four months after death ; not a single case of injury to the workmen has been known.”†

* Phil. Journal, No. 18, p. 238.

† Contrast this with the decrepitude and premature old age, brought on by living in an atmosphere impregnated with miasmatic exhalations.

‡ American Journal, No 9, p. 235

This account and the one preceding it, show in the most conclusive manner that putrefying animal matters, not only are not hurtful in themselves, but it would appear from them that they are actually endowed with a sort of preservative efficacy from the effect of miasmata. In confirmation of this, we may mention the fact stated by Rush, that of nearly one hundred butchers who remained in the city of Philadelphia, during the yellow fever of 1793, only three died; and we are told by Ambrose Parey that the plague was checked in a city in Italy by killing all the cats and dogs in the place, and leaving them to putrefy in the streets.

We will now proceed to examine some of the instances brought forward by Dr. Parsons to prove the affirmative of this question; and previous to entering upon this examination we will observe that we have no doubt that excessively concentrated animal miasma suddenly bursting out from a confined situation, may not only produce asphyxia, but also death in those immediately exposed to it. Such were some of the instances brought forward by Dr. Parsons; and we can well imagine that "in a wider circle of dilution," they may produce nausea, loss of appetite, &c., but that any thing like epidemic fever has ever originated from animal putrefaction, we totally disbelieve.

The first instance of epidemic fever supposed to have originated from putrefying animal matter referred to by Dr. Parsons, is that which occurred in New London, in 1798. This case has often been referred to by other writers. "Of the whole number of cases of fever (246) two hundred and thirty-one were clearly traced to the spot where the sickness commenced, that is, the persons were conversant, or had been in that part of the city a few days before they were seized, and scarcely a single person escaped the disorder who resided in that part of the city."

"It appears (says Mr. Channing, in his letter to Dr. Mitchell) that there was a large quantity of dried fish in a bad state, in four or five stores, within twenty or thirty rods of each other, and all in the limits referred to." "The heat of the summer having been very great, many of these fish were found to be in a moist slimy state early in August last. From a quantity lying in bulk in a store occupied by Mr. Jones, (who fell a victim to the epidemic,) a quantity of green and yellow purulent matter ran upon the floor. It was thought by the owners that if they were spread in the sun, in the open air, the fish might be preserved; which was done, extending them a considerable distance along the street and wharves. While thus exposed to the excessive heat of the sun, with light winds, the effluvia in the neighbourhood were very offensive."*

We have already mentioned an instance in which a whole cargo of

* Med. Repository, vol. 2, p. 402.

putrid cod-fish, produced not the slightest injury to the health of those exposed to its operation, though in a situation highly favourable to its deleterious operation, they being confined in very close uncomfortable quarters; we are constrained then in this case to look for some other cause of this severe epidemic.

Dr. Channing, speaking of the situation of New London, says it is elevated, "with scarcely any low grounds to generate miasma." If this expression means any thing at all, we must infer from it, that there were some low grounds in the neighbourhood. And here we suspect will be found the whole secret of the origin of this fever. The summer of this year, in the language of Mr. Channing, exceeded "in *intensity and duration* what has ever been experienced by the oldest inhabitants;" and again he says, "I am fully convinced, that had not last summer exceeded what has ever been known before, in *long-continued* and *intense heat*, with no thunder or rain, and light winds and calms, neither the fish in its bad state, nor any other subordinate cause, would have produced our epidemic."

This intense and long-continued heat was certainly well calculated to exhale Malaria from marshy grounds, and that there was ground of this nature in the neighbourhood, we cannot but infer from Dr. Channing's letter. The situation too, of the sickly parts of the city was such as to favour the approach of Malaria, "being elevated and dry, and open to the land and sea breezes." Here then we have the presence of a known cause of epidemics, (i. e. low lands,) together with intense heat, and the situation of the sickly part of the town such as to favour miasmata being driven into it by either the land or sea breezes, according to the situation of the low land in the vicinity. These considerations, together with the facts already detailed, in which a whole cargo of putrid cod-fish, with every circumstance favourable to the production of Malaria, failed to excite even the slightest indisposition, are well calculated to throw doubt at least upon the supposed origin of this fever.

But there is one other circumstance connected with the account, which we think proves pretty clearly that this fever did not originate from the putrid fish. Dr. Channing states that a captain of a coasting vessel belonging to New London "took in about twenty quintals of the fish from three stores, on the 21st or 22d of August, and sailed for Hartford. He had them packed in hogsheads there, and delivered on the 3rd. September, on board a boat bound to a town in Vermont." This circumstance undoubtedly proves that the fish were not so far gone in a state of putrefaction, as to be rendered unfit for traffic; this must be evident also from their admitting of being packed and repacked. We now leave it to every unprejudiced mind to say whether an extensive epidemic could justly be charged to the presence

of fish, so little tainted as not to be unsaleable? But again, let us pursue this putrid fish in its journey, we ought certainly to find its course marked by disease and death. It was first landed at Hartford, and there packed in hogsheads; yet no mention is made of its having produced disease in that place. From thence it was placed on board a boat and sent to a town in Vermont; and yet neither in the hands on board the boat or in the town to which it was sent did it produce fever.* Let it be recollected, that this very fish was "part and parcel" of that which was said to be producing the most fatal and extensive disease at that very time, in New London.

The Captain of this coasting vessel, a few days after his return to New London, was seized with fever, which was charged to the fish, he had conveyed away in his vessel. But if the putrid fish was the cause of his illness, it certainly would have produced it in the other hands on board the vessel, who slept with it under their very noses, and inhaled the offensive gases arising from it, with every breath they drew.

We think therefore, that the fact of their being some low-lands in the neighbourhood of New London, to generate miasma, and the state of the weather being exactly such as to favour its production and the situation of the sickly portion such that Malaria might be driven into it from all quarters, is strong evidence, in favour of the disease having originated from this source. On the other hand, the fact that a whole cargo of putrid fish, near Fort Barrancas, produced not the slightest disease in the numbers exposed to its operation day and night, connected with the other fact, that a portion of the very putrid fish, which was said to be producing violent disease in New London, was removed and carried to Hartford in the same state, and from thence sent to the state of Vermont, without producing disease along its circuit, affords strong evidence against the supposition, that the fever in New London, arose from the putrid cod fish.

The second instance of fever referred to by Dr. Parsons, as having been caused by animal putrefaction, is related by Dr. Samuel Osborn, in the Medical Repository, Vol. 1, page 210. "The patient, a soldier, was excused from duty, on account of a violent and obstinate gonorrhœa, and a lodging was assigned him in a house without the garrison, on Governor's Island; here in a few days he was attacked by symptoms of Yellow fever, and as his physician observed, in his visits, a very offensive smell in the house, he ordered the cellar under the pa-

* We are fully warranted in saying that it produced no disease along its track; because if it had, so material a fact would have been seized upon by those who attributed the fever in New London to this putrid fish, and they would have presented it as strong additional evidence in confirmation of the truth of their opinion

tient's bed to be examined, where were found three barrels of beef in a state of extreme putrefaction. So exceedingly offensive was the smell emitted from this mass of animal putrefaction, that the soldiers who were employed to remove it, were several times forced to desist, for the purpose of breathing fresh air, before they could accomplish their design. On the removal of the beef, the patient gradually mended."

To us there are two insuperable objections to this being received as an instance of the production of fever, by animal putrefaction. 1st. It is a solitary instance of fever arising from this supposed cause, while many were exposed to its full influence, without any such effect. The physician, his nurse, or nurses—the soldiers of the garrison, who doubtless had a free access to him, and especially those persons employed to remove the putrid beef, and who were therefore exposed to the immediate and concentrated influence of the gases arising from it, all escaped the disease. 2d. Dr. Gordon relates an instance (already mentioned in a former part of the essay,) of the putrefaction of 1000 barrels of beef, which did not produce disease, either in those immediately exposed to it, or in the neighbourhood. If 1000 barrels of putrid beef failed to produce fever, we cannot for a moment admit the belief that three would have that effect. We marvel very much, that with this conclusive fact before his eyes, Dr. Parsons had the conscience to pass *it* by unnoticed, and then to cite a solitary case of fever supposed to have been produced by the exhalations from *three barrels of putrid beef*.

As to the fact mentioned by Dr. Coffin, of the disinterment of a corpse after ten weeks burial, and of the consequent sickness of thirteen out of twenty of those exposed to the effluvia arising from it; it is too vague and unauthenticated to be received as evidence upon this subject. The man was killed in Orange, New Hampshire. Dr. Coffin, (if we mistake not) resided in Newburport, Massachusetts. He does not mention his authority for the account, and we should suppose from the manner in which the circumstances are detailed, that it was a mere rumor. Until, therefore, we have a more explicit and authentic account of this case, we do not think it can be received as an example of the febrific qualities of animal miasm.

Moreover, we can mention a case of disinterment after thirty-two days' burial, when from the horrible stench emitted from the body, it must have been in a state of extreme putrefaction, which was unattended by any such effects as are said to have arisen in the instance above mentioned. M. Orfila was consulted by the police, whether poison could be detected in a body that had been buried so long as thirty days? He answered in the affirmative, and was directed to exhumate a corpse and examine it in a medico-legal point of view. The body

had been buried thirty two days, and when exposed to the open air, exhaled such a horrible fœtor, that it could not be borne at two hundred yards distance. After three hours exposure, the fœtor was a little diminished, and it was determined to try the aspersion of a solution of chloruret of lime, (which had been recommended by M. Labarraque, as efficacious in removing fœtor,) the mephitic exhalations ceased immediately, and Orfilla and his assistants were enabled to prosecute their investigation for several hours without inconvenience." Here we have a case well authenticated, and striking in its character, in which no fever resulted from exhumating a corpse, the fœtor of which could scarcely be borne at two hundred yards distance.

It will also be recollected (as already quoted) that the committee engaged in France in examining into the circumstances of the Knackers operations, state that "above two hundred exhumations are made yearly at Paris, about three or four months after death; and not a single case of injury to the workmen has been known."

"Mr. Samuel Russel, of New York, in a letter to Dr. Mitchell, states, that two hundred barrels of herrings in a considerably offensive condition, were shipped in July, 1801, to St. Croix, and the market being bad, were taken to Kingston, Jamaica, where they were placed in store."

"By this time (says Mr. Russel) the fish were discovered to be fast spoiling, and advancing rapidly through the putrefactive process; the master of the store and his clerk lived and slept in a room directly above that in which the fish were now lying and corrupting. *They were both invaded by yellow fever.*"

Here is an effect certainly not commensurate with the cause; but two persons out of all exposed to the putrid emanations from the fish being taken sick. These fish when first shipped, must have been a good deal spoiled, being then in "a considerably offensive condition;" and by the time of their arrival at Kingston, "were advancing rapidly through the putrefactive process," and yet no indisposition is produced among the sailors, who inhaled the putrid effluvia arising from it night and day during the voyage, but as soon as they are landed, they are endowed with a morbid quality.

But independent of this, there is an inconsistency in the account itself, which detracts from its weight. If these two hundred barrels of herrings "were advancing rapidly through the putrefactive process" by the time they reached Kingston, why were they *stored for sale*? It involves an absurdity to suppose that their owner would encounter the expense of storage, &c., for fish which were unsaleable, and which must certainly have been the case, if they were at the time they were stored, "rapidly advancing through the putrefactive process." It would have been much better to have cast them into the ocean, to

be devoured by the "monsters of the great deep," than to have been guilty of such an absurdity. And we can hardly suppose that the merchant would have received fish which were in a state of putrefaction, to the constant annoyance of himself and of those who frequented the store.

Independent, however, of these considerations, the fact of the frequent prevalence of yellow fever in Kingston, where no such cause is ever suspected, as far as our information extends, would require a strong and irresistible concatenation of circumstances to prove its origin in this instance from animal putrefaction; it would be necessary to show clearly the non-existence of any other cause producing the disease.

The next instance brought forward by Dr. Parsons, is the epidemic which prevailed in Newburyport, in 1796. One of the causes to which this fever was attributed, was the putrefaction of the entrails of "a great quantity of fish" which had been left exposed to the air. The weather being uncommonly moist and warm, the exhalation was very offensive to the neighbourhood. In one of the houses nearest the fish offal, the three first persons were seized with the disease; and within twenty or thirty rods the greater number of its victims lived. The majority of those who recovered lived in other parts of the town. Most, if not all, who had it at a distance, had frequented the infected neighbourhood, but did not communicate the infection to their attendants."*

This fever commenced about the middle of June, and terminated towards the last of October; thus following the usual course of miasmatic diseases. This fact of the duration of the fever is important, because upon it alone we will ground our objection to the validity of its supposed cause. Dr. Parsons, inadvertently, not to say unfairly, omits to mention that when the fever made its appearance, the putrefying entrails, &c., were removed, as is obvious from the statement of Dr. Coffin, that "the committee were very careful by removing nuisances to promote purity of air in all parts of the town." Now with this statement before him, no rational mind will contend that the putrefying entrails, &c., were the cause of this disease, which did not cease for at least three months after their removal. In the instance of the ship *Regalia*, and other vessels mentioned in this essay, the fever ceased to spread immediately upon the removal of the cause, and since this fever not only continued, but like epidemics generally, probably increased, and did not terminate until between three and four months after the removal of its supposed cause, it is obvious that there must have been some other cause more latent in its nature,

* Med. Repository, vol. 1. p 504

which from this circumstance had probably escaped the vigilance of the committee.

"In Washington, a small village, containing about four hundred inhabitants, situated six miles from Natchez, and occupying a high situation, remote from any swampy ground, the yellow fever prevailed in 1828, and was unquestionably occasioned by a quantity of putrid fish and hams lodged in a grocer's store."

The history of this fever was written by Dr. Cartwright, of Natchez, and published in the April No. of the Medical Recorder, for the year 1826. It has been minutely criticised by Dr. Cooke, and we venture to say that no unprejudiced mind, after reading his remarks, will deny that they contain a complete refutation of the origin of this fever, contended for by Dr. Cartwright. We shall be altogether indebted to his able work for what is said upon this case.

It will be observed by reference to Dr. Cartwright's essay, that in the middle of the town of Washington, "is a hollow about fifty yards wide, surrounded by high ground on every side but the north-east. On that side it contracts into a bayou or ravine, which empties about four or five hundred yards off into a branch of St. Catharine's creek, which flows between Washington and Natchez."*

"In this hollow was situated the grocer's house and lot." "The earth, (says Cartwright) had been thrown up around the palings of the lot, except at one place, (left) to serve as a drain in rainy weather. This drain was choked up with weeds and trash, when I examined it during the progress of the epidemic about to be described, and served but imperfectly the purpose for which it was intended."

"In this case then, (says Cooke) the body of the town is situated in and about a bottom, closed in by high ground on every side but one; in which there is a narrow vent for the water and filth of the town, into a small stream four or five hundred yards off. The drain through this hollow is stated to have been choked up by the weeds, &c, abounding in that rich soil, so that it did not answer the purpose intended."†

"There was a succession of rains in every month in summer, and the temperature was very high." "There were in particular four days of rain in the last week of August, and three in the first of September; during which time, until the commencement of the last rain, the mean temperature was as high as 91°, at four o'clock in the afternoon, and 82 1-2° during that rain."

"The fever commenced in a week after the last rain, about the 15th of September, and increased very rapidly until the 25th; the mean

* Cooke's Treatise, vol. 1, p. 141.

† Treatise on Path. and Ther. vol. 1, p. 144.

temperature at the same hour, for fifteen days at that time being 86 1-2 degrees. It prevailed throughout the rest of September and October, during which time the weather continued clear and hot; there being rain on four days only in fifty-four, and the mean temperature of September, at four o'clock in the afternoon being 83 degrees, and that of October 80 degrees. It ceased not until after several severe frosts in November."*

"It is clear then that in this case heat, moisture, and vegetable matter were present; that is, all the circumstances known to be essential to the production of epidemics, and therefore there is not the slightest necessity for seeking for any other cause. And even if there were any difficulty in the case, how can we attribute this disease to the exhalations from two thousand pounds of bacon, so little putrid that a man wanting to purchase, examined the whole of it, to see if there was any fit for use, when more than two hundred thousand pounds of beef produced no such effect? Or how attribute this effect to two or three barrels of putrid mackerel, when a putrid whale produced none such? Or when no epidemic follows where whole fields are covered with putrid fish?"†

"One very remarkable circumstance in this case, is that the bacon when it was found to be disagreeable to the neighbours, was privately removed in one night for fear of a fine, *before the fever broke out.*"‡

"Here also, (adds Cooke) after the alleged cause is removed the effect appears, increases to a great height, affects the whole neighbourhood, and ceases not until the severe frosts of November put an end to the exhalations from the vegetable matter still abounding; not from the animal matter removed eight weeks before."§

The last instance adduced by Dr. Parsons, to show the febrific qualities of animal putrefaction, which we shall notice, is the following. "In the month of July, 17—, a very corpulent lady died at ——. Before her death she begged as a particular favour to be buried in the parochial church. She had died on Wednesday, and on the following Saturday was buried according to her desire. The weather at the time was very hot, and a great drought had prevailed. The succeeding Sunday, a week after the lady had been buried, the protestant clergyman had a very full congregation, upwards of nine hundred persons attending, that being the day for administering the holy sacrament."

The account goes on to state, "there were about one hundred and eighty communicants. A quarter of an hour after the ceremony, before they had quitted the church, more than sixty of the communicants

* Treatise on Path. and Ther. vol. 1, p. 144.

† Cooke's Treatise, vol. 1, p. 142.

‡ Ibid p. 143.

§ Ibid.

were taken ill : several died in the most violent agonies, others of a more vigorous constitution, survived by the help of medical assistance; a most violent consternation prevailed among the whole congregation and throughout the town. It was concluded that the wine had been poisoned. The sacristy and several others belonging to the vestry, were put in irons. The persons accused underwent great hardships; during the space of a week they were confined in a dungeon, and some of them were put to the torture, but they persisted in their innocence."

"On the Sunday following, the magistrate ordered that a chalice of wine uncovered should be placed, for the space of one hour, upon the altar: the hour had scarcely elapsed, when they beheld the wine filled with myriads of insects; by tracing whence they came, it was perceived, by the rays of the sun, that they issued from the grave of the lady who had been buried the preceding fortnight. The people not belonging to the vestry were dismissed, and four men were employed to open the vault and the coffin; in doing this, two of them dropped down and expired on the spot; the other two were only saved by the utmost exertion of medical talent. It is beyond the power of words to describe the horrid appearance of the corpse when the coffin was opened. The whole was an entire mass of putrefaction; and it was now clearly perceived that the numerous insects, together with the effluvia which had issued from the body, had caused the pestilential infection which was a week before attributed to poison. It is but justice to add, that on this discovery, the accused persons were liberated and every atonement made by the magistrates and clergyman for their misguided conduct."

This is truly a singular and marvellous account, and well calculated upon a cursory perusal to produce the belief, that the effluvia from the body buried in the church was the cause of the violent illness with which so many were suddenly affected. But we would ask, why, when there were about nine hundred persons present; *sixty of the communicants alone were seized with severe sickness, and this within a quarter of an hour after the sacramental ceremony.* The effluvia if present at all, must have pervaded every part of the church, and out of upwards of nine hundred individuals present, all equally exposed to its deleterious influence, but sixty individuals and *those communicants* were affected. Is not this much too partial an operation for a general cause, so virulent in its nature as to produce a sudden and dangerous illness in sixty individuals? The unexampled rapidity and suddenness of the attacks, the "*violent agonies,*" &c., are incompatible with the known effects of Malaria, for when it destroys in a few hours, its fatal presence is never marked by the "*violent agonies,*" &c., said to have been produced in this instance. There is always indeed a want of feeling, the

very sources of sensation, the brain and spinal marrow, being overwhelmed by the pressure of disease.

But there is strong reason to believe from the account itself, that the effluvia from the putrefying body, had not even escaped into the church and therefore could not have been the cause of the disease. We are justified in supposing that there was no fœtor present, because if there had been the attention of every one would have been immediately directed to its source; and if this be not the febrific principle, it at least always accompanies it, and is prominently remarked upon in every supposed instance of the sort. But no one, we are to presume, attributed the sickness to the dead body, buried one week before, for "it was concluded that the wine had been poisoned," and that this was the cause of the disease. If this supposition then be admitted, it is strong evidence against the belief, that the putrid body was the source of this violent disease.

On the following Sunday, when the people were again assembled, "the magistrate (for what reason is not mentioned) ordered that a chalice of wine uncovered, should be placed, for the space of one hour upon the altar; the hour had scarcely elapsed, when they beheld the wine filled with myriads of insects; by tracing whence they came, it was perceived, by the rays of the sun, that they issued from the grave of the lady who had been buried the preceding fortnight. Light now broke in upon their minds—the mystery was solved—the sickness was at once attributed to the effluvia which had escaped from this mass of putrefaction; but is it not very singular and unaccountable, that at this time, when putrefaction, must have progressed still further and deeper in the body, and of course the atmosphere of the church more thoroughly impregnated with the noxious effluvia arising from it, the same lamentable catastrophe, or a worse one, which the preceding sabbath had witnessed, was not at this time repeated. We are certainly to suppose from the expression "the people not belonging to the vestry were dismissed," that the congregation was again assembled on this sabbath, and yet no disease is produced in the hundreds, whom we may suppose, were present.

What was the cause of the violent disease which so suddenly manifested itself, we cannot pretend to determine; but apart from its marvelous and unauthenticated character, (for neither the time when, or the place where, nor the name of the individual who reported it, is mentioned,) there are inconsistencies in the account itself, which in our opinion clearly prove that the putrid body buried in the church was not the cause of the disease.

There are several other instances brought forward by Dr. Parsons, in his essay to prove the affirmative of this question; but they are mere isolated statements, unconnected with any detail of circumstan-

ces, and therefore not admitting of examination. The objections which Dr. Parsons makes to the "often recited accounts of Bancroft, and others," (as he terms them) of animal putrefaction, being admitted, as valid evidence on the negative side of this question, we have endeavoured to refute; whether successfully or not, others must decide; and the instances cited by him, will not, we think, bear the test of examination.

In conclusion, we would remark, that animal miasm either is, or is not, capable of producing epidemic disease. It cannot at one time possess this febrific character, and at another time, under the same circumstances be devoid of it. It follows therefore, that if even a solitary instance can be produced, where every circumstance and contingency, claimed by the most fastidious sticklers for the doctrine, as necessary to develop the morbid principle, alleged to arise from animal putrefaction are present and operating, without the production of disease, then, the presumption is a fair one, that animal miasma is not a cause of epidemic disease, and that in the instances of its supposed origin from that source, there must have been some other cause operating, which was overlooked, because less prominent and striking to the senses; that some such *unexceptionable* cases are to be found in this essay, must, we think be admitted by every one; and a number of others, sufficient to convince every unprejudiced and impartial mind, "that animal putrefaction is not a source of Malaria, and therefore is never the cause of epidemic disease."

We have thus proved the positions with which we set out: namely, "That vegetable matter *per se* in a state of putrefaction produces in abundance the morbid principle, which is the subject of this essay; and that animal matter in the same state does not produce it. When therefore, both animal and vegetable matter, in a state of putrefaction are present during the prevalence of an epidemic disease, forming by an union of the results of both processes what has been called *vegeto-animal miasmata*, the existing disease must be ascribed solely to the Malaria arising from the vegetable putrefaction because the latter, without the former is fully competent (as has been proved) to the effect produced, and *no additional effect* results from a combination of the two miasms. The phrase *vegeto-animal miasm*, therefore, is incorrect when used to designate the particular gas which produces epidemic fever; the animal miasm having "no part nor lot" in the matter.

3d. *What are the best means to prevent its formation?* This branch of the subject is of vast importance, and heretofore has been by far too little attended to. Physicians, unfortunately, for the cause of humanity, in the eager disputes about the precise nature of the fever produced by Malaria, have had their minds too much diverted from the means necessary to prevent the formation of ~~the~~ deleterious principle,

and thus have in some measure realized the fable of the dog and his shadow.

We have already mentioned that four things are necessary to the developement of Malaria; namely, heat, moisture, vegetable matter, and the contact of air; if any one of these requisites be absent, Malaria is not produced. From this fact then we may deduce many of our preventive means; for by removing any one of them we arrest the exhalation of this gas.

Vegetable matter, with the co-operation of heat and moisture will every where produce disease, hence the great necessity for enforcing the utmost cleanliness in all large cities and towns, particularly the removal of all collections of vegetable matter. The streets, yards, cellars, ponds, gardens, stores, &c., should all be made to pass through the most rigid inspection. Much good has resulted from following this course, and dreadful distress has been occasioned by neglecting it. Dr. Rush observes, "in a manuscript of the life of Doctor, afterwards Governor Colden, of New York, there is the following fact: It was first communicated to the public in the daily Gazette of the capital of that State, on the 30th October, 1799." "A malignant fever having raged with exceeding violence for two summers successively in the city of New York, about forty years ago, he communicated his thoughts to the public on the most probable cause of the calamity; he published a little treatise on the occasion in which he collected the sentiments of the best authority, on the bad effects of *stagnating waters, moist air, damp cellars, filthy stores and dirty streets*. He showed how much these nuisances prevailed in many parts of the city, and pointed out the remedies. The corporation of the city voted him their thanks, adopted his reasoning, and established a plan for draining and cleaning the city which was attended with the the most happy effects."*

The United Provinces of Holland, (says Rush) hold their exemption from the plague only by the tenor of their cleanliness. In the character given by Luther of Pope Julius, he says "he kept the streets of Rome so clean and sweet, that there were no plagues nor sickness during his time."†

Ponds of water should never be allowed to exist in cities or towns, or even in their neighbourhood. In such situations they too often become the reservoirs of offal matter of every description; which either by gradual accumulation, may finally rise above the surface of the water, as happened in the instance related by Senac, or by evaporation of the water, may become exposed to the action of a hot sun, and in either event may exhale miasmata in abundance.

* Rush's Inquirers, vol. 4, p. 135. † Ibid p. 134.

But as we have before observed, marshes, strictly so called, are the most fruitful sources of miasmata. These abodes of this pestiferous air, from their great extent and other circumstances, will often not admit even of any attempt to change their nature; and all that is left us in such cases, is to interpose between them and us, the barriers to the progress of Malaria with which nature has kindly furnished us, and which will be mentioned in a future part of this lecture.

Though, however, in many cases no favourable change can be effected, in numerous other instances much has been done to redeem marshes from their noxious state. The experience and industry of man have often triumphed over these laboratories of Malaria, and from having been shunned as the foci of disease, they have been converted into the abodes of health and plenty. By a judicious use of ditching, so as to drain the soil of its superabundant water and subsequent cultivation, by which the earth, being frequently turned up to the drying action of the sun and air, wonderful changes have been effected. "With the aid of the purifying sea breeze, this course, (says Ferguson,) at the British colony of Demarara, within six degrees of the equator, has succeeded in rendering the cultivated portion of the deepest and most extensive morass probably in the world, a healthy, beautiful, and fertile settlement."*

The *Campagna di Roma*, now the abode of pestilence, was once comparatively healthy, and was indebted for it in a great measure to the "constant tillage," and "to the extreme attention paid to draining the deposits of stagnant water," and to the aqueducts which then traversed it in every direction. "These, however, being broken down and destroyed by the Goths, by which the country became overflowed, it soon became excessively sickly.

"In the neck below Philadelphia, since it has been drained, its agues have disappeared."†

"Fourteen miles above Calcutta, at Barrackspore, the position is healthy, but it is owing to the ground being in high cultivation, and cleared and drained all around to a great extent. On the opposite shore of the Hoogly, at Serampore, for the same reason, the climate is salubrious. But above all, at Chandernagore, about five miles further up, on the west side, the health of Europeans is proverbial; but there the French have taken great pains to drain the grounds. The position chosen for the settlement is elevated above the bed of the water at high tide above fifty feet, and those ditches, about which so much was debated in the treaty of peace, of 1763, and which the British were so apprehensive of being converted into a military fosse, actually drain off vast bodies of water for four or five miles from the

* Phil. Journal, No. 13, p. 20.

† Med. Recorder, July, 1825, p. 493.

river. These ditches are admirable evidence of sagacity and indifference to expense, as the ditches are lined and bottomed with the finest bricks, and convey to those volumes of water into the main river which were before suffered to stagnate and infect the air. Ohinsurah, two miles further up, is a healthy position, and here great pains were also taken to drain off the rains.”*

These examples present in a striking point of view, the good effects of draining marshy land by extensive ditching, &c., by means of which it is brought into a fit state for cultivation. Without this latter effect, however, no good is effected, for so long as the land is too wet for the plough, it will under certain circumstances exhale miasmata. Draining and cultivation should therefore always go hand in hand, the first being generally nugatory without the co-operation of the latter.

Many persons are deterred from attempting to rescue their lands from a marshy state, by the trouble and expense necessarily attending it; but health should certainly be preferred before all other considerations, and independent of the attainment of this grand object, the bountiful crops yielded by lands thus redeemed, will most frequently more than compensate for all the trouble and expense incurred.

If, however, the situation of the marshy land is such as not to admit of draining, &c., there is one other expedient, whereby to prevent the exhalation of miasmata, of which we can avail ourselves in certain situations, and that is to inundate the marshy soil, and thus protect its surface from the action of the sun. This expedient will not only certainly prevent the formation of Malaria, but when forming, will immediately arrest its further production.

Sir John Pringle informs us, that the inhabitants of Beda, in Dutch Brabant, prevent the exhalations which give rise to fevers, by inundating the low grounds in the neighbourhood; and “Purchas tells us, (says Rush) 500 persons less died of the plague the day after the Nile overflowed the grounds which had emitted the putrid exhalations that had produced it, than had died the day before.”†

The clearing of wet land, should be followed up immediately by cultivation. Land of this description having on it a growth of timber, should never have its wood cut down without the specific purpose and power immediately to devote the soil so cleared to agricultural uses; because experience has taught that the *mere clearing* of marshy land, is almost certain to be followed by disease in its neighbourhood. In proof of this, Rush says, “it has often been observed, that families enjoy good health, for many years, in the swamps of Delaware and North Carolina, while they are in their natural state, but that sickness always follows the action of the sun upon the moist surface of the

* Lind on Hot Climates, p. 65.

† Inquiries and Obser. vol. 4, p. 129.

earth, after they are cleared. For this reason the cultivation of a country should always follow the cutting down of its timber, in order to prevent the new ground becoming, by its exhalations, a source of disease.”*

Dr. Robert Jackson also says, that “in this country, (America,) the unhealthiness of a place is often obviously increased, by cutting down the woods of the neighbouring swamps; therefore, no rule is more liable to exceptions, than that which has been so generally enforced, namely, that clearing a country of its woods invariably renders it healthy; unless the grounds be drained and cultivated as well as cleared, the effect is likely to be the reverse.”†

4th. *When formed, what are the measures necessary to remove or render inert the sources of it?* Common sense would at once suggest the propriety of removing all obvious causes of disease, which from their nature will admit of their removal. Thus heaps of decaying vegetable matter, may and certainly ought to be removed; and where fevers arise from this source, they will cease to spread simultaneously with the removal of the cause. We have striking instances of the immediate efficiency of a removal of the morbid cause, in putting a stop to fever, in vessels in which it prevails, as soon as the holds are cleansed and purified, it ceases.

But when Malaria is exhaled from extensive marshy ground, all attempts to put a stop to it for the time being are vain, unless it is so situated as to admit of being flooded, which as we have before said, is immediately effectual in producing this desirable event. The only alternative that is left, is to remove from the neighbourhood of the marsh until a kindly frost shall have stopped the exhalation, when persons may generally return to it with safety. But a repetition of this unpleasant and with many impracticable alternative, may be prevented by ditching and cultivation, where the circumstances of the ground admit of the employment of those remedies. But where they do not a very effectual barrier against the approach of Malaria, may be obtained by the interposition of trees or a wood between the marsh and the affected spot. This to be sure is a slow method, the advantage of which we may not reap for years, but it is at the same time a sure one. The trees or wood, no doubt, act mechanically in obstructing the approach of Malaria, since a high wall or a house has the same effect.

Many places, previously healthy, have become immediately sickly upon cutting down an interposing wood between them and a marsh, and on the other hand, many situations before sickly have been rendered healthy from the growth of trees between them and marshy grounds. Dr. Jackson says, “histories abound with examples of

* *Inquiries and Obser.* vol. 4, p. 136.

† *On Fever*, p. 55.

destructive epidemics, which have followed the cutting down of groves, which covered morasses or intercepted the approach of Malaria."*

Dr. Lisle in his memoir on Malaria, translated by Dr. Johnson, and incorporated in his work on Tropical Climates, says "upon Mount Argintal, above the village of St. Stephano, there is a convent which has lost all the reputation for salubrity it once enjoyed, since the lofty trees by which it was surrounded have been cut down."†

He also says, "I have been informed by persons worthy of credit, that in consequence of felling the wood before Asterna, near the Pontine marshes, Velettri was visited for three successive years by disease which made much greater havoc than usual throughout the whole country."‡

Again, he says, "I have seen the poor fishermen, who had taken up their abode near the canal, which runs from Campo Salino to the sea; they had built their huts close to a wood that screened them from the direct access of the infected winds which pass over that morass; and declared that they never suffered inconvenience from them so long as they remained under that shelter."

He states the following fact from Volney, "Barrant, formerly very unhealthy, has ceased to be so since the Emir Fakr-el-din planted a wood of fir trees, which still exists a league below the town. The monks of Mark-anha, who are not systematic natural philosophers, have made the same observations respecting different convents."§

These instances, and they might be multiplied, plainly shew the effects of wood or grove of trees in arresting the progress of Malaria, and ought as well to warn us, never to destroy a wood happily situated between us and a marsh, as to teach the salutary lesson to supply it when absent by planting trees, &c. This may seem to some so slow a method of attaining the desired object, that they may on that account neglect to avail themselves of it, but though we may never enjoy the benefit of it, we should have an eye to posterity, and not selfishly refuse to confer a blessing upon our children, because we could not live to enjoy it, and thus leave them estates cursed with unhealthiness, which leaving the danger to life out of the question, withers and dries up all the sources of earthly comfort.

5th. *What are the best means of obviating its effects upon the human constitution, when the cause cannot be removed?*

These means may, we think, be properly divided into such as operate upon the constitution, and into those which are furnished us by our knowledge of some of the characteristics of Malaria. Among the most important and beneficial of the first division may be men-

*On Fever, p. 54. †On diseases of Trop. Climates, vol. 2, p. 122. ‡Ibid. §Ibid

tioned a proper regimen. This we think, if judiciously pursued, might be rendered very efficient, if not in preventing, at least in ameliorating the effects of miasmata; and in our opinion, has never yet been presented in its full value and importance. Entertaining this belief, we regretted very much, to find, upon perusing Professor Caldwell's essay upon febrile miasm, and his more recent prize essay on the same subject, that he disapproved of any change from the accustomed regimen, with a view to counteract the effects of Malaria. "Not a little (says he) has been said about diet, drink, and general regimen best calculated to protect those who are exposed to febrile miasm. Some recommend a very moderate diet, consisting chiefly if not exclusively, of vegetables, with water as the only drink. The object of this is stated to be so to purify the blood, that the miasm may find in it no suitable matter on which to act. Others urge the propriety of a free, if not a full diet of animal food, with a liberal allowance of wine, or some other stimulating beverage, with a view as they express it, "to live above the fever."

"The rule of wisdom (continues he,) appears to be as follows.—Let those who are exposed to a febrile miasm, persevere steadily in their usual course of diet, drink and regimen, provided they have found it by experience to agree with them; I mean provided it has secured to them the greatest amount of health, strength, and general comfort of which they are susceptible. For in proportion to these will be the resistance of their systems to the action of poison, any thing that may in the least derange their health or weaken their powers, increases necessarily their liability to disease." "During the prevalence of an epidemic, then, let those whose habit it is to eat animal food, and drink wine or spirits in moderation, continue to do so; while those whose diet has been vegetable, and their potation aqueous, should persevere in them; provided, I say, they have found them salutary."* With all our deference and high respect for the opinion of Professor Caldwell, for no one do we entertain more, we conscientiously believe that the above extract is fraught with error, and contains advice not only dangerous, but of fatal tendency. It may be abused so as to afford direct and positive encouragement to the voluptuary, to continue in his course of living, regardless of the solemn warnings of death, which in sickly times, may constantly cross his path. Ask, the devotee of the pleasures of the table whether he finds his course of living injurious or not to his health, and in nine cases out of ten, you will be answered in the negative, aye, and with truth too, very often; for free living and drinking, which unfortunately too often go by the name of moderate indulgence in such pleasures, (if pleasures they can be call-

*Transylvania Journal, Nov. 1830, p. 513.

ed) and which misnomer lulls their unhappy victim into fatal security, may be enjoyed for years without any *perceptible* effect upon the health and constitution; they may be surely though with sure and progressive steps, undermining the very citadel of health, and no external beacon be held out to warn the victim of the work going on within; until at last the bloated face, the indurated liver and swelled abdomen, tell too plainly of the wreck of his constitution. We need not say that such individuals are not only more liable to fever, but have it in a more aggravated shape than others.

Neither reason, fact, or analogy, we think will support Professor Caldwell in his notion as to the regimen most proper to obviate the effects of Malaria. We would ask him, what it is that renders the "inoculated small pox" so much milder than the "natural small pox?" Surely not the bare insertion of the poison into the arm; but rather the low diet, the frequent cathartics, and the refrigerating treatment which are used to "prepare the system" for the disease. By this course of preparatory treatment, small-pox, even before the discovery of vaccination, was in a great measure robbed of its terrors,* and we have not the shadow of a doubt, that could it be known when the contagion was received into the system, the same course of preparation would be equally effectual in mitigating the symptoms of the "natural small-pox." Why then, should not the same plan, be equally useful in rendering milder the operation of Malaria? Both are invisible poisons floating in the atmosphere; the primary symptoms produced by both are alike, and we have no doubt that both destroy life through the same means, namely by insufficiently controlled excitement, together with the ravages of local inflammation.

But independent of these considerations, we have positive and unquestionable evidence in favour of abstemiousness in rendering milder the effects of miasmata. Dr. Jackson remarks, that the French and Spaniards, who eat "less animal food," and drink their liquors greatly more diluted than the natives of England, escape the diseases of tropical climates much better than the latter; and their safety I might add, has been remarked to bear some proportion to the different degrees of abstemiousness, which they are known to observe. An idea prevails, (continues he) with the generality of people who visit warmer or more unhealthy climates, that it is necessary to eat and drink freely, as a security against the attacks of endemic fevers; but a very narrow observation will serve to show, that good living as it is called has no such effects, and we may even soon perceive, unless blinded by long established prejudices which flatter our appetites, that it is actually at-

*It was estimated that only one in two hundred died, of those who were inoculated for the small-pox.

tended with pernicious consequences. The most abstemious, as far as I have observed, escaped the best, not only from the attacks, but particularly from the danger of disease.”*

Again, he says, “it is known to every medical man, that the fevers of hot climates are generally most dangerous in full and plethoric habits. It ought to be an object of attention, therefore, to obviate this cause of mortality by spare living, and the cautious use of stimulating liquors.”

Dr. Rush, in his history of the Yellow Fever, of 1793, affords striking evidence of the good effects of low diet, both in preventing and mitigating the operation of miasmata. In a publication, (says he) dated the 16th of September, I recommended a diet of milk and vegetables, and cooling purges to be taken once or twice a week, to the citizens of Philadelphia. This advice was the result of the theory of the disease I had adopted, and of the successful practice which had arisen from it. In my interview with my fellow-citizens, I advised this regimen to be regulated by the degrees of fatigue and foul air to which they were exposed. I likewise advised moderate blood letting to all such persons as were of a plethoric habit. To men, (continues he) who were influenced by the publication in favour of bark and wine, and who were unable at that time to grasp the extent and force of the remote cause of this terrible fever, the idea of dieting, purging and bleeding the inhabitants of a whole city, appeared to be extravagant and absurd. But I had not only the analogy of the regimen made use of to prepare the body for the small pox, but many precedents in favour of the advice. The benefits of low diet, as a preventive of the plague were established by many authors, long before they received the testimony of the benevolent Mr. Howard in their favour.” “By means of the low diet, gentle physic, and occasional bleedings, which I thus publicly recommended, the disease was prevented in many instances, or rendered mild when it was taken.”†

In another place, he says, that by following the above advice, “the progress of the fever was restrained in many families, or it was rendered when taken as mild as inoculation does small pox.”

These facts are strong, and certainly outweigh the mere naked and unsupported opinion of Professor Caldwell.

The regimen then, which we would strongly recommend to all persons during the prevalence of a fatal epidemic, is such as will lower the inflammatory tone and diathesis of the system and remove or diminish plethora; a diet consisting of vegetables and water alone, for drink is precisely calculated to have such an effect, without in the least degree impairing the strength or weakening the powers of the system: nay, we

* On Fever, p. 258.

† Inquiries and Obser. vol. 3, p. 160.

believe it increases its powers of endurance. If this be true, as we will endeavour to prove, if a regimen consisting essentially of vegetables and water for drink, diminishes the plethora and inflammatory tendencies of the system; and at the same time preserves the strength, it cannot but be beneficial; and any course of living having a tendency to keep up or increase this state must be prejudicial, hence we deprecate the use of much or any animal food, and alcohol in any shape, be it either in the nauseous potation of grog, or the more fashionable glass of wine. This regimen is not recommended with the object in view mentioned by Dr. Caldwell, namely, "so to purify the blood, that the miasm may find in it no suitable matter on which to act;" this, we consider absurd and ridiculous; but because its effect is to diminish plethora, and to subdue inflammatory diathesis if present; in fine, to put the system in such a condition as will best enable it to withstand the shocks of disease.

We have said that a regimen such as we have recommended, so far from enfeebling the powers of the system, added to its capabilities of endurance. We cite Dr. Jackson as our authority. Speaking of a journey he performed while in the West Indies, he says: "I left Kingston, on foot, about twelve o'clock, and accomplished a journey before it was dark, of eighteen miles. I did not find I was materially fatigued, and still persisting in my resolution, travelled a hundred miles more in the space of the three following days. It may not be improper to remark that I carried baggage with me, equal in weight to the knapsack of a soldier."

In the next page he says "I observed formerly, that abstemiousness and temperance were among the best means of preserving health, or obviating the danger of the diseases to which troops are liable on their first arrival in hot climates; but the rules of temperance are little regarded by English soldiers at any time, and almost constantly transgressed, whenever extraordinary labour is required of them. To such causes of excess, joined with the great heat of the sun, we may attribute many of the bad effects of marching, or immoderate fatigue, in the West Indies. In the journey which I have just mentioned, I probably owe my escape from sickness to temperance and a spare living. I breakfasted on tea about ten in the morning, and made a meal of bread and sallad after I had taken up my lodging for the night. If I had occasion to drink through the day, water or lemonade was my beverage. In the year 1782, I walked between Edinburgh and London, in eleven days and a half; and invariably observed, that I performed my journey with greater ease and pleasure, where I drank water, and only breakfasted and supped, than when I made three meals a day, and drank wine, ale, or porter."*

*On Fever, p. 266.

Johnson, speaking of the Hindoos, remarks, that "the lower or industrious classes on the other hand, who live almost exclusively on vegetables, certainly have a striking resemblance to "Pharaoh's leaved fleshed kine." But though they have not the physical strength of an European, they make up for this, in what may be termed "bottom;" for it is well known, that a native will go through three times as much fatigue, under a burning sky, as would kill an Englishman outright.—Witness the palankeen bearers, coolies, dandies, hircarrahs, &c."*

These facts show plainly that a vegetable diet is amply sufficient to sustain the strength under the severest exercise, and evince we think the futility of the objections to adopting a regimen such as recommended, upon the ground of its being too "sudden and great a change in diet and drink, and such as would tend to weaken the system."

It may be thought perhaps, that we have devoted too much space to the consideration of regimen; we do not however think so. The subject upon which we lecture is not confined in its bearing and interest to a single community, it is co-extensive with the prevalence of Malaria; and thinking as we do, that a proper regimen is one of the most powerful means of mitigating its operation, we could not without doing injustice to the subject have said less. What enhances its importance, too, is, that it admits of easy and universal application, and if generally followed during severe epidemic visitations, would unquestionably detract greatly from their mortality.

Professor Caldwell, is also unfriendly to the use of medicines, bleeding, &c., as a means of prevention and amelioration of the effects of Malaria; but in this he is equally erroneous and unsupported by facts, as in the former. Dr. Rush considered bleeding very useful, particularly in the robust and plethoric, in warding off, and in mitigating the fever excited by Malaria. As precedents in favour of the practice he cites the authority of Dr. Haller, and Dr. Hodges, who both found it very beneficial during the prevalence of plague. Dr. Rush gives a remarkable instance of its good effects: It was communicated to him in the year 1798, by Dr. Boland, physician to the British army. The account is as follows: "In the beginning of August, 1797, 109 Dutch Artillery, arrived at Port-au-Prince, in the Bangalore Transport. The florid appearance of the men, their cumbersome clothing, and the season of the year, seemed all unfavourable omens of the melancholy fate we presumed awaited them. It was however thought a favourable opportunity by Dr. Jackson and myself, to try what could be done in warding off the fever. It was accordingly suggested to Monsieur Conturier, the chief surgeon of the foreign troops, and the surgeon of the regiment, that the whole detach-

*Tropical Climates, vol. 2, p. 256.

ment should be blooded freely, and that the morning after a dose of physic should be administered to every man. This was implicitly complied with a day or two after, and at this moment in which I write, although a period of four months has elapsed, but two of that detachment have died, one of whom was in a dangerous state when he landed. A success unparalleled during the war in St. Domingo. It is true several have been attacked by disease, but in those the symptoms were less violent, and readily subsided by the use of the lancet."

"The crew of the Bangalore, on her arrival at Port-au-Prince, consisted of twenty-eight men. With them no preventive plan was followed. In a very few weeks eight died, and at present, of the original number, but fourteen remain."*

We believe the use of medicine, so as to preserve the bowels in a laxative state, has proved very beneficial in warding off, and rendering milder, the operation of Malaria; the same reason which would induce us to recommend a spare diet, would also suggest the use of mild medicine for the above mentioned object. It is a powerful means of both removing and preventing plethora and all local accumulations and determination of blood. Many writers mention their salutary effect. Dr. Edward Miller, in his observations on Yellow fever, says, "of all the means of obviating the approach of this disease, preserving regularity in the intestinal discharges, is perhaps, the most important. And, when indeed, this is completely done, it seems probable that nothing short of the most concentrated and virulent assault of miasmata will be sufficient to bring on diseases."†

"Hundreds, perhaps thousands, of the citizens of Philadelphia (says Rush) were indebted for their preservation from the Yellow fever, to the occasional use of a calomel pill, a few grains of Rhubarb, or a table-spoonful of sweet, or castor oil, during the prevalence of our late pestilential fevers. Even the air of Batavia,‡ has been deprived of its poisonous qualities by means of this class of medicines. A citizen of Philadelphia, asked a Captain of a New-England ship, whom he met at that island, how he preserved the whole crew of his ship in health, while half of the sailors of all the other ships in the harbour were sick or dead. He informed him, that it was by giving each of them a gentle purge of sulphur every day."§

It would seem from some very remarkable facts on record, that calomel taken so as to excite slight ptyalism, exercises a preservative efficacy from the diseases produced by Malaria. Dr. Rush says it preserved most of the crew of a Russian Ship, at Plymouth, in the year 1777, from a fever generated by filth in her hold."

*Inquiries and observ. vol. 6, p. 118.

†Med Repository, vol. 2 p. 393.

‡Batavia is probably one of the most sickly places on the globe. It has been computed that one half of all now-comers die there on the first year of their arrival.

§ Inquiries and Observations, vol. 4, p. 117.

"In a letter (says he) which I received from Capt. Thomas Truxton, in the year 1797, he informed me, that an old and respectable merchant at Batavia, had assured him, he had been preserved in good health by calomel taken in the way that has been mentioned, during the sickly seasons, for upwards of thirty years."*

But the most astonishing fact in proof of the prophylactic powers of calomel, occurred at the fatal island of Edam, already mentioned. The surgeon, who wrote the account of the fever which destroyed so many at that place, after describing the particulars of it, remarks: "One circumstance more is so singular in itself, and so much attracted our notice at the time, that I think it deserves commemoration. *Of all the people or patients who slept at the fatal island of Edam, four only, to the best of my knowledge, escaped the fever entirely, and returned to Malacca. These were two obstinate venereals, and two chronic dysenteries; all under the influence of mercury, for some time before I sent them to the hospital. Their complaints did not get better in the least on shore, so that they continued to take mercury there. They slept in the same ward with the fever patients all the time, but never had the slightest symptom of fever themselves.*"†

All the prophylactic measures mentioned, (with the exception perhaps of the last,) namely: spare diet, occasional bleeding, and medicine, owe their beneficial effects to the same principle of action; they all reduce the quantity of the circulating fluids, and we contend that fever supervening under such circumstances, will be less high, less inflammatory, and therefore less dangerous, than under opposite circumstances: there may be a fulness or exuberance of health (if the expression is allowable) which will not only invite disease, but which will render it vastly more severe than it otherwise would have been: such persons possess more of the pabulum of fever, and hence it is that in them we often witness a violence and rapidity in the march of disease, which frequently defies all our efforts to arrest it.

Persons exposed to an atmosphere vitiated by Miasmata, should avoid as far as practicable, fatigue or exhaustion either by bodily labor or walking, or remaining much in the hot sun. Fatigue often proves a powerful exciting cause of Miasmatic diseases, developing them in all their fearful activity, and destroying life in a few hours. Dr. John Hunter says, that in persons fatigued by hard labor and long fasting, the poison gains admission more readily into the system, and produces

* Inquiries and observ. vol. 6, p. 121.

† Dr. Johnson, in remarking upon the above occurrence, says; "It accords with my own experience, for I have not known a person fairly under the influence of mercury, for the cure of any other complaint, to be attacked either by endemic or contagious fever." And in allusion to the escape of the mercurial patients at Edam, he says, "such an immunity cannot be attributed to chance. The proofs are both positive and negative. *They, and they only, escaped the fever.*"

immediately the worst kind of fever. It is in this way (he adds) that soldiers suffer so much on actual service in the West Indies: the few cases of fever which proved *fatal in twenty-four hours*, that occurred to me, were all contracted in a similar manner.”*

Cold particularly, when united with dampness, is also a strong exciting cause of disease in those whose systems are impregnated with Malaria. Lind mentions particularly the pernicious effect of the damp, cold easterly wind in increasing the number of the sick. An undue exposure to it therefore, should be avoided as far as possible.

The depressing passions, such as fear, grief, chagrin, despondency, &c. are powerful auxiliaries to Miasmata, in producing disease: while the opposite feelings of hope, that “star of life’s tremulous ocean,” which shines though with sometimes dimmed, yet never extinguished lustre, over our path through life, “brace the system as it were against the attacks of Malaria.

Johnson mentions some striking instances of the opposite effects produced by these opposite states of feeling. “I have indeed (says he) remarked that most of those, who were of a timid disposition, and easily alarmed at the prevalence of the endemic diseases of the country, fell under their influence sooner than those of a contrary temperament. But grief, disappointment, and chagrin, were the depressing passions which universally induced the most decided and unequivocal predisposition to disease. I saw many strong and melancholy instances of this among that part of our crew, which we impressed within sight of their own shores, and probably of their own habitations, when we were commencing our voyage to India. They were among the first and worst cases I had under my care, and afforded ample proofs, that mental despondency can accelerate the attack, and render difficult the cure of intertropical fevers in particular. I have seen the influence of this predisposing cause on a large scale;—not on the banks of the Ganges, but, much nearer home, on the banks of the Scheldt.”

“When our army (continues he) lay entrenched under the walls of Flushing, without any other defence from the sun, the rains and dews; than some brushwood or straw;—generally, indeed, with the humid earth for their beds, and the canopy of Heaven for their curtains; still, with all these disadvantages, the animating prospect of success, the mental energy inspired by *hope*, united with corporeal activity, kept the whole army in health. When Flushing surrendered, however, and another object was not *instantly* held out for pursuit or attainment, a fatal pause took place, and a kind of torpor, or rather exhaustion ensued, during which the remote cause of fever, namely, *vegeto ani-*

* Diseases of the Army in Jamaica.

mal miasmata, began to make some impression. But when from the ramparts of Batz, we clearly discovered with our glasses, a strong boom crossing the Scheldt from Fort Lillo,—the surrounding country in a state of inundation, and various other insuperable obstacles between us and the “*ulterior objects*” of the expedition; then, indeed, the depressing passions, and some other predisposing or exciting causes, communicated a fearful activity to marsh effluvia, which rivalled in its effects any thing that I had seen in tropical climates.”*

Dr. Rush, in speaking of those causes which counteracted the effect of medicine, and increased the mortality of the yellow fever of 1793, mentions as one of them, “the universal depression of mind, amounting in some instances to despair, which affected many people. What medicine, (says he) could act upon a patient who awoke in the night, and saw through the broken and faint light of a candle, no human creature, but a black nurse, perhaps asleep in a distant corner of the room; and who heard no noise, but that of a hearse conveying, perhaps, a neighbour or a friend to the grave?”

We could easily multiply such instances as these, but they are sufficient to show the injurious operation of the depressing passions upon those exposed to miasmatic exhalation. These causes, no doubt, operate extensively both in increasing and rendering more violent the cases of disease, in every malignant and severe epidemic. Few, unsupported by religious hopes and aspirations, have fortitude enough to behold with calmness and resignation, the fearful ravages of death among relations and connexions, without a feeling of agonizing distress, a gloomy foreboding of ills yet to come, and alarm for personal safety. These feelings so natural, and we may say, so honorable to us, should be struggled against as much as possible; grief and despair being too of the most powerful co-operatives with Malaria in the development of disease.

We will now mention those means of obviating the effects of Malaria, which are furnished us by our knowledge of some of its properties. The first rule we would suggest under this head, and it is one of cardinal importance too, is by all means to avoid the evening or night air, as well as an exposure to the atmosphere of the morning previous to sunrise. The reason of this rule will be obvious, when it is recollected that miasma is dispersed by heat of the sun, during the day, and that as soon as he withdraws his beams and sinks below the horizon, it begins to descend, and remains in the lower strata of atmosphere, until it is again rarified and dispersed by his kindly rays. The instances already cited, show conclusively this characteristic of Malaria, and will impress more strongly than any words of mine can do, the

* Diseases of Tropical Climates, vol. 1, p. 118.

necessity and advantage of the caution suggested. The fatal island of Edam, among many others, is a memorable instance of the deleterious operation of night air charged with this morbid principle, as well as the complete dispersion of it by the heat of the sun during the day.

De Lilse says, "the evening dew is so much dreaded at Rome, that as soon as it begins to be perceived, all the inhabitants shut themselves up in their houses:" and again, "the people of Italy, and I suppose of all countries where the air is bad, never go abroad, unless absolutely obliged, till after sun rise, when the heat has dispersed the pernicious vapours that have fallen during the night."*

He gives a striking and curious example of the good effect of avoiding the night and early morning air. "In one of the most unhealthy corners of the Pontine marshes (says he) I found a man who had for several years been employed there in making charcoal from turf. During this period, he had never been afflicted with any disease, and when questioned respecting a circumstance so very extraordinary in such a place, he ascribed the preservation of his health, to the following precautions: He made a particular account of returning to his hut by sunset, where they kept a continual fire; he never left it again till late in the morning, and remained near his furnaces in the day time. It is obvious that miasmata either did not penetrate into his hut, or if they did, the vapours combined with them were rarified by the heat of the fire, and carried off by the currents of air, which this fire incessantly produced. This man so well instructed in experience, had a florid complexion, and a totally different look from the people of the country, who, taking no precautions, are annually exposed to a mortal disease, and drag on a truly pitiable existence."†

In the next place we would recommend, that the upper apartments of houses should be used for sleeping; experience clearly establishing the fact, "that the lower you are, the denser are the strata of miasmata," and consequently the more violent the effects arising from it. We have already mentioned several examples in proof of the superior salubrity of the second stories of houses, and need not repeat them here.

The practice of seclusion, keeping the doors and windows shut, so as to oppose a mechanical obstacle to the ingress of Malaria, has been found useful and is well worthy of a trial by those at least whose circumstances will admit of it. De Lilse strongly recommends it." If I had (says he) to direct the inhabitants of a town attacked with alarming epidemic disease, * * * * I would immediately enjoin the ge-

* Johnson on Tropical Climates, vol. 2, p. 120.

†Ibid. p. 121.

neral seclusion of all the citizens. I would enforce the order by the point of the bayonet; and until the purity of the atmosphere should appear to me to be completely restored, public functionaries should supply the wants of the inhabitants, and keep up such communications, as are indispensably necessary."*

The burning of a number of fires so as to disperse the Malaria, by the artificial heat thus created, has often been used with good effect, in warding off its attacks. Heat produced in this way, no doubt operates in the same manner as the heat of the sun, dilating and rarifying the gas in question, and forming an upward current, by which it is carried off.

We have now concluded our remarks upon this very interesting subject. We might have lengthened them very much, but our wish and aim has been to be as brief, as was deemed consistent with its value and importance.

†Johnson on Trop. Cli. vol. 2, p. 123.

